ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY ARISING FROM THE USE OF ASBESTOS IN ONTARIO

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Mr. Ublansky, Atomic Energy and Chemical Workers

180 Dundas Street
Toronto, Ontario
Thursday,
June 11, 1981

VOLUME VIII

ROYAL COMMISSION ON MATTERS OF HEALTH AND SAFETY ARISING FROM THE USE OF ASBESTOS IN ONTARIO

VOLUME VIII

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ERRATA AND ADDENDA: References to Dr. Hans Wile should read Dr. Hans Weill

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Thursday,
June 11, 1981
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THE FURTHER PROCEEDINGS IN THIS INQUIRY
RESUMED PURSUANT TO ADJOURNMENT

APPEARANCES AS HERETOFORE NOTED

DR. DUPRE: Good morning, ladies and gentlemen.

Today the Commission begins a phase of its hearings at which it will be hearing sworn testimony from a variety of witnesses. Our first witness, who I wish to greet most warmly this morning, is an expert witness from the University of Pittsburgh, Dr. Philip Enterline.

Dr. Enterline, sir, may I say that you are most welcome here. We are very, very grateful for your willingness to come at considerable inconvenience to yourself to appear before us this morning to give sworn testimony. You are welcome indeed, sir.

Miss Kahn, would you swear in the witness, please?

MR. LASKIN: Perhaps, Mr. Commissioner, before she does that I could just introduce to you my colleagues who are here.

DR. DUPRE: If you please.

MR. LASKIN: On my far left is Mr. Jim McNamee, who is representing the Government of Ontario. Jim, could you tell us who is with you?

MR. McNAMEE: Murray Finklestein, Department of Labour.

MR. LASKIN: Then in the back row on the far left is Mr. David Starkman, who is acting for the Asbestos Victims of Ontario; Miss Linda Jolley, who is acting for the Ontario Federation of Labour and the various parties with standing under that umbrella; and then Mr. Ed Warren, who is with us from Washington and who is acting for the Asbestos Information Association of North America, and with him is Mr. Tim Hardy, who is one row behind; and then Mr. Jean Bazin from the Quebec Asbestos Mining Association.

Have I missed anybody?

Mr. Ublanski is also with us from the Atomic Energy and Chemical Workers.

I think I've got everybody, Mr. Chairman.

DR. DUPRE: Thank you.

So these are the parties with standing, who will be cross-examining the witnesses after you have finished your direct examination?

MR. LASKIN: Some may not, I understand, but they have worked out between them, as I understand it, some arrangement to question.

DR. DUPRE: Thank you.

Miss Jolley and gentlemen, you are all very welcome here, indeed.

Miss Kahn, would you swear in the witness, please.

<u>DR. PHILIP ENTERLINE, SWORN</u>

<u>EXAMINATION-IN-CHIEF BY MR. LASKIN</u>

Q. Dr. Enterline, you are presently a professor of biostatics and the chairman of the department at the University of Pittsburgh?

A. Yes, I am.

Q. I gather you have been at Pittsburgh since about 1967?

A. Right.

MR. [illegible]: Then in the back row on the left is Mr. David [illegible], who is acting for the [illegible] of [illegible]; [illegible], who is acting for the [illegible] Federation of [illegible] and the [illegible] with [illegible] and then Mr. [illegible], who is [illegible] and who is acting for the [illegible] Association of North America, and [illegible] who is [illegible] from the [illegible].

[illegible]

MR. [illegible]: [illegible] and [illegible].

Q. [illegible].

MR. [illegible]: Thank you.

[illegible], who will be cross-examining [illegible] after you have finished your direct examination.

THE [illegible]: [illegible], as I understand it, [illegible].

MR. [illegible]: Thank you.

[illegible] and gentlemen, you [illegible] here, indeed.

[illegible], would you swear in the witness, please.

DR. PHILIP [illegible], Sworn

EXAMINATION-IN-CHIEF BY MR. [illegible]

Q. Dr. [illegible], you are presently a professor of linguistics and the chairman of the department at the University of Pittsburgh?

A. Yes, I am.

Q. I gather you have been at Pittsburgh since about 1961?

A. Right.

Q. As the chairman indicated, for a brief period of time you were at McGill University?

A. Two years.

Q. You have a master's degree and a doctorate from an American university?

A. Yes.

MR. LASKIN: Let me file, Mr. Chairman, if I might, a brief which contains Dr. Enterline's complete curriculum vitae, including a list of literally hundreds of publications which he has, a number of which are in the asbestos field.

If I might file this simply as exhibit number one, and I think I have recirculated copies of the index so that if we refer to any of the articles in this brief, we will refer to them by their tab number.

I'll just put that in front of you in the event that you need it.

EXHIBIT # 1: The abovementioned document was then produced and marked.

MR. LASKIN: Now, Mr. Chairman, we may proceed a little bit out of the ordinary with respect to Dr. Enterline for this reason, that very recently, within the past two months, Dr. Enterline has delivered two papers which, in my submission, are of relevance to the mandate of this Commission.

Neither of those papers have been published. Dr. Enterline has been kind enough to bring them here today. He is prepared to leave them with us and is prepared to discuss them with the use of projection equipment. It seems to me before we get into any specific questioning it would be most helpful to have Dr. Enterline talk about those two papers.

MR. LASKIN: Q. Can you, for everybody's sake, just tell us, DR. Enterline, briefly, what those papers address?

THE WITNESS: A. Yes. The first paper addresses the question of what proportion of all cancer can be attributed to asbestos exposure, and it arises from the government document, a U.S. government document, that claimed somewhere between thirteen and eighteen percent of all cancer due to asbestos, in the United States.

The second paper addresses the question of whether using epidemiologic methods we could ever test the two fiber standard now in effect in the United States. It is a kind of a quasi-statistical paper, but I think does produce an answer to that question of whether epidemiologic methods are going to be useful in dropping the standard or changing the standard from the present two fibers.

Q. Good. How would you like to do this?

A. Well, maybe I'll just present these kind of papers in tandem. This might take...I think they were twenty minute presentations...it might stretch out a little bit, and I don't know, you can ask questions as we go along.

I'll give you a little bit of background about each paper. You will see that they obviously tie together. I think if you write two papers two months apart, they are going to sound a lot alike because you are thinking about the same kinds of things, and if you do work on one subject, you would like to use it in both papers if you can.

So you will see, I think, some similarities. But I think that the issues, as I understand the purpose of this Commission, that I address are probably issues of interest to the Commission, and so these papers might be useful to you.

MR. LASKIN: Let me say for everybody's benefit that I am having these papers xeroxed and they will be available to you as soon as the xerox machine produces them.

MR. LASKIN: Q. All right?

THE WITNESS: A. Well, let's take the first one,

A. (cont'd.) and that's the one that addresses this question of what proportion of cancer...and this is in the United States, but it would apply, I think, to Canada...what proportion of cancer is due to exposure to asbestos.

I think everyone realizes that cancer is a very important cause of death in the United States. If the current levels of cancer incidents continue, we can estimate about fifty million people alive today in the United States will die of cancer.

The problem in the field of prevention and public health is simply, what can we do to alter this prediction and is there some way we could make it forty-nine million or forty-eight million, or something less than what we now...than the fifty million, we think, deaths might perhaps occur.

Now, of course, the United States government and the National Cancer Institute is very concerned about this, and they would like to know what is the best strategy, what is the best way to spend public funds to alter this prediction.

A couple of years ago, there appeared a widely-circulated but unpublished document that estimated that between thirteen and eighteen percent of all cancer in the United States is caused by a single substance - asbestos. That would be nice to believe. Nice in the sense that it would tell us what we should do. That would be very directive for efforts in government, I think, to know just what to do.

Of course if that were true, I think the obvious thing to do would be to ban asbestos. Not only ban it, but get rid of all the asbestos we now have in our environment.

But a lot of very thoughtful people, in reviewing this document, disagreed, some very violently, and there were a number of papers published subsequently that did disagree with this.

I was asked a few months ago to prepare an estimate

THE WITNESS: (cont'd.) of what proportion of cancer is due to asbestos, present this at a meeting held at Coldsprings Harbour, Long Island, New York, at a conference on the quantification of occupational cancer.

There are a lot of approaches you can take in making an estimate like this. One approach could be to find a kind of index disease, a disease that is certainly caused by asbestos, and then calculate the ratio of that disease to all cancer that is caused by asbestos.

We'll give you an example: We are pretty sure that most malignant mesotheliomas are caused by asbestos, and some people think that for every mesothelioma that occurs there are perhaps one point five, one and a half...if you can conceive of that...of bronchogenic cancers that occur due to asbestos.

So that if you know then how many mesotheliomas there are occurring in a population, you would know how many lung cancers occur in that population, due to asbestos. So that if mesotheliomas are caused by asbestos, the theory would go we could then estimate how much cancer, more generally how much lung cancer, is caused by asbestos, and you could extend that and say, well, how many other cancers in addition to lung cancer are there related to mesotheliomas, what is the ratio of other cancers.

All of this leads to an estimate of how much cancer is caused by asbestos. Start with the index case, find out the ratio of that to all other cancers, simply multiply that ratio by the number of index cases and you can calculate the number of all cancers caused.

A second approach could be to actually estimate how many people are exposed and calculate the level of exposure, determine the dose-response relationship between asbestos and cancer, and make a direct estimate of how many cancers are being caused. If you know the level of exposure, you know the dose-

THE WITNESS: (cont'd.) response relationship, then you can calculate the number of cases produced by that level of exposure.

There have been a number of papers that have used that approach.

The approach I used is essentially a mix of these two approaches. My paper deals with first looking at an index disease, index kind of cases, and then secondly, looking at the size of the exposed population, the level of their exposure, and calculating another estimate of how much cancer that population of that size at that level of exposure, how much cancer will be produced.

The index disease that I have used is not mesothelioma, but asbestosis. This is a disease by definition that is caused by asbestos. The name asbestosis arises from the notion that this is a disease caused by the inhalation of asbestos fibers, and this is a disease that does lead to death and perhaps is just as fatal as lung cancer. It is a disease that has been recognized for many years. In fact, we recognized asbestosis as a consequence of asbestos exposure long before we recognized cancer as a consequence.

So we have a long history of reporting of asbestosis cases, and it's a well-recognized cause of death in the national vital statistics system in the United States, and certainly in the system in Canada.

So, we know how much asbestosis there is, we know how many people die of it, and that should tell us something about the size of the population that's producing these asbestosis deaths.

Back in the early 1960's, I did a study of a very large part of the population in the United States engaged in primary asbestos products production. In other words, these are the people that made the asbestos textiles, made brake linings,

THE WITNESS: (cont'd.) made asbestos cement products, and I was able to identify, back in the early 1960's, about twenty-one thousand male workers in the United States who had been producing asbestos products. These were workers that had been employed some time during the years 1948 to 1951, and I just simply traced these people to see what their mortality experience was.

Let's show some slides now that will put these two ideas together - my study together with the idea that asbestos causes deaths, and we know how many deaths are caused in the United States by asbestos.

The first slide deals with the numbers of deaths due to asbestosis, in the United States, going back to 1950. So this is kind of...oh, boy, the first slide is missing.

All right. I'll tell you what the first slide would have said if I hadn't forgotten to bring it. It would have shown that numbers of deaths due to asbestosis rise from about eight in 1950, to somewhere around thirty-five or forty in the 1970's, and it shows that there was a gradual increase during the fifties and sixties, and then starting in 1970, kind of a plateau - not much happening.

So think of this - that's not very many deaths for a large country with two hundred and twenty-five million people in it, due to asbestosis, but remember, this is a recognized disease, it has a high fatality rate, but it suggests that if you only get thirty-five or forty deaths per year in a large country that there couldn't be a very big population out there producing those deaths.

Okay, now, put that together with this second idea, and that is that I studied, in fact, about twenty-one thousand asbestos product workers for mortality, and I know for that population how many deaths were being produced.

THE WITNESS: (cont'd.) Now, on the slide you see... this study I just mentioned...you see that I have twenty-one thousand, seven hundred and fifty-five asbestos product workers at ages under sixty-five, and over a period about 1950 to 1963, there were forty-four asbestosis deaths in my cohort.

So, twenty-one thousand people produced forty-four deaths over about a thirteen year period.

Now, in the United States in the same period, if you summed across all the years, 1950 to 1963, you would find in the whole population of the U.S. there were a hundred and eighty-four thousand deaths. I'm sorry - one hundred and eighty-four deaths in the entire United States.

Now, it's not very difficult to calculate from that how many people it would have taken to produce a hundred and eighty-four deaths. If you know that twenty-one thousand produced forty-four deaths, how many people would it take to produce a hundred and eighty-four deaths?

Well, you divide forty-four into a hundred and eighty-four and multiply that by twenty-one thousand and you get the answer to that.

Well, of course, my first study that I mentioned dealt only with people under the age of sixty-five. I then did a study of people who were sixty-five and over. There were a thousand, seventy-five of these. They produced seventeen deaths, and we might talk later about that particular study. It's a study of retired asbestos workers.

During the same period in the entire United States there were five hundred and four deaths being produced, and again it wouldn't be hard to calculate from that how many people there were over the age of sixty-five producing the five hundred and four asbestosis deaths.

All of this leads, then, to an estimate of how many people, what is the size of the population in the United

THE WITNESS: (cont'd.) States that produces asbestosis deaths. From my two studies, I calculate sixty-two thousand, seven hundred and five...just by those ratios that I have described to you.

Now, as a check on this, I looked at another study by Irving Selikoff, where he studied seventeen thousand, eight hundred asbestos insulators, and he did the same kind of thing I did. He identified these insulators, then he followed them over a period of years and he observed in his insulators seventy asbestosis deaths, and during the same period in the entire United States there were three hundred and seventy-one occurred. And once again, if you divide seven into three seventy-one, multiply that times the seventeen thousand eight, you come out with a number of eighty-four thousand, nine hundred and six.

So what this says to me is that, certainly in the United States during the fifties and sixties, the population that produced these deaths from asbestosis had to be less than a hundred thousand people. One estimate is sixty-two thousand, one estimate is eighty-four thousand.

Now, this begins to give you some idea of the dimensions of the problem as expressed in terms of exposed workers. Somewhere less than a hundred thousand, okay?

Now, I think it has the advantage in that the estimate arises from two studies, one of asbestos products workers and the other of asbestos insulators. And incidentally, these two populations really represent, in my view, all of the really heavily exposed workers in the United States, and I think this might be true in other parts of the world. We don't have mining in the United States, so I can't speak about that, we don't have any extensive mining such as you have in Canada, but I think that if you talk about heavily exposed workers, the number must be something less than a hundred thousand, in the United States.

THE WITNESS: (cont'd.) Now, I can double check on this easily. Let's see the next slide. Let's see how many insulators are there.

Well, first of all let's look at the bottom half of the slide. We know how many insulators there are. First of all, the unions report their membership to the U.S. government. They report somewhere between twelve and eighteen thousand as an average membership during the 1950's and 1960's. The U.S. census counts asbestos insulators, it's one of the definable occupations in the census, and the census enumerates between fifteen and twenty-five thousand insulators.

Now, of course, these are numbers, looking at the bottom half of the slide now, these are numbers that represent an average census. That at any one time this is how many workers there are, but of course what we would like to know is, how many different workers, how many different people were exposed over a long period of time, how many different people are there. Because, of course, the membership might be twenty thousand at one point in time, of a union, but of course over a period of ten years there might be forty or fifty thousand different people who belong to the union and who get exposed.

So let's extend the exercise now and say, if the average population is of these sizes I've shown here, can we go from there to an estimate of how many different people...let's say who are alive today in the United States...how many different people are there who were heavily exposed to asbestos.

I define heavily exposed as being either an insulation worker or a worker in a primary asbestos product - brake linings, insulation material, textiles and so forth.

Now, I do a lot of studies of worker populations and I usually start with something like an average census. Somebody comes to me and says, "I have a factory, it employs a thousand people, would you tell me whether my factory is

THE WITNESS: (cont'd.) producing, whether my workers have an excess of disease".

The first thing I have to know is, not how many people you have on average, but how many different people, how many different people have worked for this particular company... many of whom have left, many of whom have died...how many different people over a period of time.

What I've learned from that is, that I can calculate the number of different people by multiplying the average census by three, and I come out pretty close. That's what I have essentially done here. I've said, well, if the average number of people...now I'm talking about over the period since World War II, let's say...if the average number of people is somewhere around twenty thousand, about twenty thousand asbestos products on average, and about twenty thousand insulators on average, then the number of different people exposed a year or more...and I should have added that...in my studies, those exposed a year or more, is three times that, or about sixty thousand.

So, my estimate is that as of 1970, in the United States, there were..well, as of 1970...no, I'm sorry. Back up.

During the period 1940 to 1970, in the United States, my estimate is that there were a hundred and twenty thousand people heavily exposed to asbestos during that thirty year period - a hundred and twenty thousand different people.

Now, of course, if we would like to know how many people are there now alive who have been heavily exposed to asbestos, we are going to have to subtract from that hundred and twenty thousand the people who died during the period 1940 to 1970, and I've applied some life table methods here and I calculate that as of 1970, there were ninety thousand people alive who had been...

MR. LASKIN: Q. 1981?

THE WITNESS: A. I beg your pardon?

Q. 1981?

A. No, as of 1970.

And I did something else I probably would be criticized for. I said, well, what happened in 1970? Why 1970? Well, that's when the federal government started to regulate and set standards, and I said well, let's suppose that the government was really successful and that after 1970, there were no more heavily exposed workers...that is, they stopped heavy exposure and so I'll say well, let's just let that ninety thousand rest and say that's true in 1981, that nobody, in effect, nobody was added to this pool.

In other words, what I'm saying in fact is, I'm trying to make an estimate of the number of heavily exposed workers at the end of...as of 1970...and I'm saying that because of government regulations there are no more people added to that pool of heavily exposed workers after 1970, and so I'm not adding any more heavily exposed workers. I think I probably did have to account for a few deaths that occurred from 1970 to 1980, but the bottom line is that in 1981, my estimate is that there were alive in the United States ninety thousand people who had a history of heavy exposure to asbestos.

Now, what does that have to do with the estimate I want to make? Well, of course, if I want to know what proportion of cancer in the future can be attributable to asbestos, one of the first elements would be, well, how many people are there available to contribute cancer to the total cancer incidents? How many people are available to make a contribution to all the cancer that occurs in the next ten or twenty or thirty years in the United States?

I should make it clear, I suppose, that when we say what proportion of cancer is due to asbestos, we have to specify a little bit what we mean by that. I think the intent of the government document was to say that during the next ten,

THE WITNESS: (cont'd.) twenty, thirty years, they estimated that thirteen to eighteen percent of all cancer that would occur in the United States, that will occur in the future, will be due to asbestos. So I'm trying to make my estimate fit with that. I'm trying to say that of the cancer that will occur in the United States during the next two or three decades, what proportion will be due to asbestos. To do that, I've got to decide right now how many people are alive who were heavily exposed. If I know that, then I have the basis for making an estimate of their contribution to the cancer problem. I estimate ninety thousand.

Okay, what am I going to do with the ninety thousand? Well, I have to get some estimate of the risk these people carry with them, the risk they picked up as a result of their exposure, and calculate for the ninety thousand what proportion will die as a result of their exposure to asbestos, and more specifically in this case, will die of cancer as a result of their expcsure to asbestos.

Okay, well, I've dealt now just with heavily exposed workers in the United States. I have defined them, where we can get them, I've kind of verified my numbers are about the right magnitude, but there are a lot of other people exposed.

There are a lot of people exposed outside of the primary asbestos products industry, and outside of the asbestos insulators, and let me deal with that next now, about these other people that are exposed.

Let's see the next slide. Okay, let's pursue this calculation, and this is what I've just talked about. I'm talking about those alive in 1981. There's my eighty thousand.

Is that what I've got on the other slide, eighty thousand?

MR. LASKIN: Ninety, I think.

THE WITNESS: Ninety. I think I sort of, if you read my paper, I had to fine tune the ninety. I had some reasons for dropping it to eighty, which I would have to go back and read.

MR. FINKELSTEIN: I think you had ninety alive in 1970.

THE WITNESS: Okay. No, they were alive in 1981, that previous slide.

I think I dropped it as kind of a compromise, because I think I had a couple of estimates there.

MR. WARREN: Eighty is what is used in the paper, though.

THE WITNESS: Eighty comes out finally in the paper, okay. Well, there is a reason for that and you'll have to read the paper to figure it out. It is a kind of a compromise number. We are still under a hundred thousand. I don't want you to think this is...

DR. UFFEN: Is it a compromise with that sixty thousand you had in the beginning, as the first estimate?

THE WITNESS: I think maybe, I think maybe I did that.

DR. UFFEN: By the other method?

THE WITNESS: I think I said...yeah, perhaps that's how I got that. Yeah.

Okay, now you can see what I did. I calculated for the insulators that twenty-two percent would die of...I call this asbestos lung cancer, meaning that it's lung cancer due to asbestos, to differentiate it from all the other lung cancer that occurs...in the next thirty years, and a somewhat lower proportion of the products workers will die of asbestos lung cancer.

Now, this is something we might want to talk about later, why a different estimate for these two groups, but I'm

THE WITNESS: (cont'd.) basing this...this is total empiricism, I'm just basing this on published studies...and there definitely is a difference in the incidence, in the relative risk, let's say, of lung cancer in insulators as compared to asbestos products workers, and I have just recognized that here.

MR. LASKIN: Is the twenty-two percent based on Selikoff's...

THE WITNESS: A. It is really based on his insulators study, right.

Q. That's the figure he came up with?

A. Yes, yes.

Q. And the fifteen percent is the figure that you came up with in respect of the insulators?

A. Pretty much. My figure is fourteen percent. The fifteen percent actually comes out of the study that was done by Julian Peto, who you will hear from, I understand, later. But mine...my retired workers is fourteen, Peto's is fifteen. Again, I choose fifteen...you don't like to quote yourself too much and if somebody agrees with you, you quote them and say gee, isn't that great, we are about together on this thing.

Well, from that you can get the deaths per year that will be produced by this heavily exposed population. Now, again these numbers don't add up - two ninety-three and two hundred do not add to five thirty, and there is a reason for that, and you'll have to read...again, it's described in...maybe there is no point in going into that. There is a reason for that. You'll see it if you read the paper, but it's a little technical and it wouldn't be easy for me to explain right now.

So, I calculate eighty thousand alive, twenty percent will die as a result of the asbestos exposure, die of lung cancer, which is five thirty a year.

Now, you see we are beginning to get someplace now, because if there are four hundred thousand deaths from

THE WITNESS: (cont'd.) cancer in the United States each year, right away we say, well, gee, the contribution of the heavy asbestos exposure is only five hundred and thirty out of the four hundred thousand, so right away we are nowhere near fourteen percent or those numbers.

But let's move on to the other populations that we could look at. Let's see the next slide.

There has been a lot of talk in the U.S. about World War II shipyard workers and there are two studies by a statistician named Blot, B L O T, in which he claims that the work in the shipyards during World War II, and the asbestos exposure that took place, have resulted in very large numbers of cancers in the United States, and in fact when the U.S. government made its estimate of thirteen to eighteen percent, they relied very heavily on this study by Blot.

There they are, that first line deals with the shipyard workers. But one of the things about these workers was that their average exposure was only a year. The real activity in the shipyards was only in the years 1943, 1944 and 1945. When the war ended the shipyards just went dead. They had a tremendous letout, so very few people actually worked all three years. Many people only worked a few months. There was, as you could imagine in wartime, there were a lot of people in and out of employment, into the army, out of the army, and so forth, and there are some studies that show that the average worker, the average duration of employment was only a year.

But there was a sizable exposure. This comes to the second way of making an estimate. Now what I have to do, since the shipyard workers apparently produce little, if any, asbestosis, in order to decide how much lung cancer is being produced I am going to have to know what their exposure is and invoke some kind of a dose-response notion here.

What I have to calculate now is, what would you

THE WITNESS: (cont'd.) expect a year's exposure at two fibers per c.c. to produce in the way of cancer? Now, in the way of cancer, I mean in the way of cancer starting in 1981.

Of course, these people will be past middle age now, so you have to take that into consideration. In any event, what I have put on the slide simply is what I could find out about their exposure.

Now, how do I know what their exposure was? That's the toughest part. What I did was, first of all there is nothing in the literature. This exposure was never really measured, and I started just calling industrial hygienists who worked in shipyards and just kind of, you know, telling them what the problem was and trying to get some guess. All I can say is, these guys are fairly informed people, and the best guess I could get was about two fibers, on average. Now, there are some people who were exposed very heavily and some people who were distant from where the asbestos was used who might have been exposed lightly. I would say this is an informed guess.

I have one other piece of information, however, and that comes from Johns Hopkins University. Genevieve Mattenowski, a professor of epidemiology there, is doing a study of shipyard workers in the United States, and she has obtained from a large number of shipyards all their industrial hygiene data.

I happened to be on her advisory committee, so I have access to all this industrial hygiene data, and I also used that to make these estimates of the exposure levels, and that's in the paper too, when you get a chance to read it.

So I do know something about shipyards. Granted, it's anecdotal, partly, and granted, many of the data I have are recent and not historic, but these are my guesses.

Now, you notice that after World War II, obviously employment dropped. I calculate two hundred and eighty-five thousand people went to work after World War II in shipyards,

THE WITNESS: (cont'd.) and are alive in 1981. They were exposed for a fairly long period because they were sort of permanent employees, but their exposure was lower. Again, I got this the same way I got the two fibers.

A third population that we talk a lot about are auto mechanics. These will be working in garages where there is brake lining operations going on, and I can get an estimate of these of seventeen...one million, seven hundred thousand, and there is pretty good data on auto mechanics. There is good industrial hygiene data. I am very confident of that point three fibers per c.c. as an average exposure. I got some good references on that.

Now, a very interesting population of secondary asbestos workers - these are people that are taking asbestos products and making other products from them. For example, there are workers who take asbestos cloth and make asbestos gloves from the cloth. One of the characteristics of this segment of the industry is that it tends to be small, highly competitive and probably poorly controlled. I think those people have pretty good exposures and I have some good data on that. It comes from a study that was commissioned by the Asbestos Information Association, and I calculate their exposures at four fibers per c.c.

Now, all other workers - this is where it gets really tough. Now, these would be people in construction, carpenters, for example, who might work near somebody who is using asbestos to insulate pipes, that sort of thing. That's a fairly large population. I simply said that they probably have an experience like auto mechanics, and I've simply used the point three which I can document, but when I applied that to all other workers, admittedly that's just saying by analogy perhaps they have exposures like auto mechanics. But we don't know too much about it.

THE WITNESS: (cont'd.) Okay, now I know...okay, now we get the other part of the estimate. Now we can estimate the amount of cancer produced by exposure not as heavy as the exposure of the primary products workers, and the exposure the insulators have.

Let's see the next slide.

Here is...now, once again using these kind of proportions, who will die as a result of their exposure that I showed you two slides back. I've applied these to those alive in 1981, and I get a deaths per year for each of these populations, and notice that these secondary asbestos products workers are producing a lot of deaths. That's a very important population and I think it's totally overlooked by most people. But these World War II shipyard workers...while there's a huge population... produce relatively few deaths because they were exposed very briefly, and we believe, I believe, that exposure is a time weighted measure. I think that a year's exposure to two fibers is like two years exposure to one fiber, sort of thing, in terms of its end result - cancer, in this case.

Okay, so we can add all those up, and let's see the last slide. You can't read that real well.

Well, now I've just brought together all these different slides...gee, that's terrible. Okay, here's the five thirty for the insulation and primary asbestos products workers, the secondary asbestos products is producing even more - nine hundred. You remember I pointed out that is an important population that people tend to ignore.

There is the World War II shipyard workers, the post-World War II shipyard workers, the auto mechanics, and then the all other group about which we are probably the weakest.

Add this all up and you get twenty-five hundred and one...is that what it is...cases, deaths per year during the next twenty-five or thirty years in the United States.

THE WITNESS: (cont'd.) What's that? Other asbestos exposures...?

MR. LASKIN: Cancers.

THE WITNESS: Cancers, okay. Of course I've only talked about lung cancer so far. I believe that asbestos does produce other types of cancer, and I believe that perhaps for every two lung cancers, there is one other kind of cancer produced. These could be digestive cancers, and I think they are primarily digestive, but it also includes...yes, these are primarily digestive cancers. I think that perhaps the evidence is good enough that we can conclude that there is an excess in digestive cancers caused by asbestos. I've put those in there.

Now, what about the mesotheliomas? These are the pleural and peritoneal mesotheliomas that I spoke of earlier. Most people think that these are nearly all caused by asbestos. My guess is that of about a thousand of these that occur each year in the United States, only about a third are occupational. About a third, I feel, are caused by asbestos, but not in an occupational environment. These could be children whose father was an asbestos and they have been exposed as a result of his bringing the asbestos home in his clothes, and I believe that about a third are not due to asbestos. I'm not sure what they are due to, but we have enough experience in other parts of the world with this tumor to know that asbestos is not the sole reason for these.

So I've taken the thousand cases that occur in the United States each year and attributed a third to occupational exposure, three hundred and thirty-three, and the bottom line there is, four thousand, eighty-five deaths per year during the next twenty to thirty years which occur in the United States will be due, cancer deaths, will be due to asbestos.

Now, remember that I said earlier that there are

THE WITNESS: (cont'd.) about four hundred thousand deaths a year in the United States, cancer deaths, and four thousand is just one percent of four hundred thousand.

Now, obviously this is very different than the government estimate.

Of course, having done all this work I couldn't resist moving on and saying well, how much cancer is caused by nonoccupational exposures. I already had estimated that about a third of the mesotheliomas are nonoccupational, and that's the bottom line...two hundred and twenty-five million people in the United States producing three hundred and thirty-three...that's a third of the thousand occur each year.

Well, what about the asbestos lung cancer? We do know something about the ambient exposure levels in the United States. The best...my estimate is that it runs about five nanograms per cubic meter across the whole country, would be a good estimate of what the air concentration is - five nanograms. That's not very much. That's a billionth of a gram. It's a nanogram per cubic meter. My guess...is that twenty-eight? What is it?

MR. LASKIN: Twenty-eight.

THE WITNESS: Twenty-eight, yes. My guess is, every year in the United States there are twenty-eight people die of lung cancer as a result of this ambient exposure. I must say I've cranked in here the schoolhouse exposures that you probably know about, the hair dryer exposures that you may have read about, and so forth. That's all cranked into that twenty-eight, a very, very small amount. That's kind of an addendum on the slide and actually is only because once I got all that momentum up with the four thousand, I couldn't stop and I had to go on...and having calculated all these formulas...apply this to the whole population of the United States.

So to repeat the bottom line, it is simply that I

THE WITNESS: (cont'd.) think that the contribution of asbestos to our cancer problem is rather small, one percent. If asbestos were removed totally, there would be no detectable effect on the overall cancer problem, there would clearly be an effect on the incidence of mesothelioma, and clearly an effect on deaths from asbestosis, these thirty-five or so deaths that occur a year eventually would disappear.

I think that's important though. I think that it would be a mistake to allocate too much of our national resource to a single substance as the government document I referred to earlier suggests we might do, and then find out in twenty or thirty years that it didn't make really any difference.

I think now is the time to be realistic. I think the trouble is that somehow people think they are helping the public by overestimating things. Maybe in the short run that might be so. Maybe in the short run the way to get people's attention is to make some fantastic, absurd estimate. It gets a lot of publicity, certainly in terms of the U.S. Congress you get a lot of committees looking at the thing and perhaps some increase in the appropriation, but I think in the long run that's very bad policy.

What I've tried to do is give my best estimate. I haven't even given ranges. This is my best estimate. This is what I think is the best estimate you can make about the contribution of asbestos to the cancer problem in the United States.

Anybody have any questions about this?

MR. LASKIN: Q. Why don't you tell us briefly about the second aspect of your recent move in relation to lung cancer and the two fiber standard, and then we'll come back and give some specific questions.

THE WITNESS: A. Okay.

MR. LASKIN: Do you have copies?

THE WITNESS: A. Okay. Well, the other paper, the background of it was that I'm the associate director of the Center for Environmental Epidemiology. This is an EPA-funded center, Environmental Protection Agency-funded center, that is located at the University of Pittsburgh.

The focus of that Center is on what contribution can epidemiology make to the identification and solution of problems dealing with ambient air pollution in the general population, nonoccupational by and large.

Well, the...since my orientation is largely towards occupational environments, I decided to address the question of - could epidemiology make a contribution towards setting a standard for an occupational group. I picked asbestos because I knew a little bit about it, and tried to answer the question, could epidemiology provide any evidence on the safety of the present standard of two fibers per c.c.?

Or, is the...now, why couldn't it? Well, one of the reasons you might not be able to do much with epidemiology is, the amount of disease produced at that level, the disease increment produced by two fiber standard is too small to detect by methods, epidemiologic methods, which are admittedly pretty crude methods.

Some people claimed, for example, that perhaps epidemiology could not detect excesses less than doubling effects. In other words, if something less than doubled existing disease incidence, perhaps epidemiologic methods are really too imprecise to even pick that up. That's kind of an extreme view. I think perhaps epidemiology might pick up one and a half-fold risk. You know, there's a fifty percent excess, you might pick it up epidemiologically.

There are a lot of reasons for that. Maybe you are aware of them and we won't talk about them at the moment.

Okay, let's just look then at the problem. Can...

MR. BAZIN: Sorry. Could we have maybe three minutes to take cognizance of this document before Dr. Enterline gets into it? We just...

MR. LASKIN: We are all in the same position, so why don't we just...we'll carry on.

THE WITNESS: This will only take a few minutes.

MR. LASKIN: It will only take a few minutes.

THE WITNESS: This really bears on the document, incidentally, and I think maybe what I have to say will help you a little bit with that.

Okay, let's see some slides on this and see how I did this thing. Well, I first, since the paper had to do with setting standards, this slide just simply shows how the standards have dropped in the United States, and deals with the various OSHA proposals, standards, NIOSH recommendations and so forth. The only important thing is that you see we went from ten million particles per cubic foot in 1930, to five in 1938, and about 1968, we made the switch from particles to fibers and then we started to express the standard in terms of fibers and you see where it went from twelve to five to two and the proposal now...well, there are several proposals, but one is that they drop to point one.

Okay, let's see the next slide here.

Now, in order to decide whether you can pick up an effect of two fibers, you have to decide how big that effect is. In order to decide how big the effect is, you have to decide what is the dose-response relationship.

I only show this...this is picked up from the Simpson report, which I'm sure you've all read, this is the British Advisory Committee on Asbestos. They had some very nice slides in here, and they simply showed some different kinds of relationships there could be between exposure to asbestos and some kind of response.

THE WITNESS: (cont'd.) I think the only ones of real interest are the upper line, the two on the right, the middle one on the upper line is the simple linear relationship, that's proportional excesses, for every increment in asbestos, there's a corresponding increment in, in this case, in lung cancer.

Then, of course, the one on the extreme right on the upper line is one some people believe in. This is certainly true in the linear quadratic relationships in radiation. They believe that it's not a straight line, but it curves upwards something like that one you see on the right, and much of the argument is between those two, although in one of my early papers I actually fitted the one on the lower right, accumulative normal distribution...although if you read my papers you find out that my line kept getting so straighter and straighter, so I finally fitted the one into the top middle. Just a little discussion of the different ways that a substance might relate to a particular response.

Let's see the next slide.

Well, just to think a little bit about asbestos and lung cancer, here is a slide that relates to the study that was done by McGill University in the Province of Quebec. In fact, I took this from a paper by Geoff Berry. I suspect you'll see this again, but he had some very nice pictures in his paper and I copied this from his paper, but it simply shows that the little squares represent actual data points, and you can use your imagination as to which of those lines on the previous slide best fit those data points.

Most people think that the straight line is pretty good, and it does fit pretty well except at the upper end, and you'll hear more about that upper end, that's a curious thing that shows up on all studies as kind of a dropping off of response at very high levels. But the lower end of the thing, where we are really interested in the lower end anyhow, because

THE WITNESS: (cont'd.) we are setting standards at much below the levels that exist on this slide. Perhaps it is linear.

The next slide is simply...it's amazing. Okay, do it again.

Okay, this is a study I did of retired asbestos workers. You see...I don't know if you could fit an S curve to that or not. Originally I did, but as time went on, people said well, gee, it isn't an S curve, it probably is a straight line. Perhaps it is. Anyhow, I've...Geoff Berry fitted a straight line to my data and it doesn't fit too badly, except that the lower end...there is a little confusion as to what is going on down there. Perhaps it's...

DR. UFFEN: Excuse me. As a matter of clarification, it's a little hard for me to see, but at zero exposure is there a finite response?

THE WITNESS: There was an excess. In my study, if you extrapolated in the early study, you would extrapolate to something above zero response.

DR. UFFEN: For zero exposure?

THE WITNESS: Yes. Now, I should say, the latest study I have extrapolates exactly through zero. Not forced, but actually extrapolates through it.

Most writers, when they do this, will force the line through zero on the grounds that zero exposure ought to give zero response. I haven't forced any of these things, so in that case, you see, it doesn't come out exactly at a hundred.

A hundred in this case would be zero response. That's the SMR for lung cancer. If it's a hundred, it means that the observed number of lung cancers was exactly the same as what might have been expected.

Anyhow, this is an area for considerable debate even yet, I think. What is the dose-response? Is it straight line?

THE WITNESS: (cont'd.) I've just, to sort of skirt that debate, I've just assumed that it is a straight line relationship in the calculations I'm going to make in just a minute.

Okay, let's see the next slide.

Ah, now here's a dilemma. You realize that if you had a straight line, there are only two parameters to be estimated. One is the...it's what is called the Y intercept, which we just discussed. In other words, at what point does a line intercept the vertical axis. Well, of course, if you believe that zero dose ought to give zero response, you can wipe that one out. You say well, of course it has to intercept: at zero dose it has to intercept at zero response. So then the only other parameter is a slope.

Now, you notice I've got a little formula down there called SMR equals a hundred plus a hundred B. Well, the only parameter I've put down here is a slope, a slope being what do you multiply in this case times a hundred?

Now, why do I have a hundred? Well, the current standard of two fibers is derived from the British two fiber standard that said that this is, quote, safe, acceptable let's say. Two fibers is acceptable after fifty years exposure.

In other words, if you think that response is a function of dose rate to fibers, times time - fifty years, fifty years work - then we would express the dose as a hundred, fifty times two or a hundred.

In other words, a hundred fiber -per-c.c. years is saying, in effect, that these are workers exposed at two fibers for fifty years, so you get a hundred.

So that's how I've got a hundred in that formula down there - a hundred fiber-per-c.c. years.

Now, the dilemma here is that these slopes are very, very different. The lowest slope is the one that Claude MacDonald at McGill derived, and which is quoted in the Simpson report, which I refer to as the British Advisory Committee on

THE WITNESS: (cont'd.) Asbestos here, BACOA. The highest, steepest slope comes from a very recent study by the National Institute for Occupational Safety and Health, called the Dement Study, of a look at four...I mean a fantastic, unbelievable difference among those slopes.

Now, what I've given in the last column is what the predicted relative risk would be for lung cancer after fifty years exposure at two fibers, if that slope were in fact true. So opposite each one of the slopes I've got the predicted excess, really, of lung cancer that corresponds to that slope.

So that you would read that as MacDonald says that if his data are right, that after fifty years exposure to two fibers you would get a six percent excess. You can read the SMR's that the death rate would be a hundred and six percent of the expected, which is another way of saying a six point two percent excess in lung cancer would result from an exposure at two fibers per c.c.

Now, most of the studies are kind of in the ballpark of MacDonald. Now, there is a Peto study, a new Peto study, which I described in my paper and you might ask him about this, that gives a seven point eight percent excess. My study, also in the Simpson report, is a twenty-one percent excess.

Hans Wile, you will hear from him, his study predicts something close to mine - twenty-nine percent excess.

The Simpson report on the earlier Peto study is a fifty percent excess, if you can read it that way.

But the Dement study is a four hundred percent excess. It is simply off the scale, just simply hard to comprehend how studies could come up with answers so different.

Well, to go back, what is the problem I'm trying to solve? I'm trying to find out whether epidemiologic methods would pick up, are sensitive enough to pick up an effect of exposure at two fibers. Obviously, my answer depends on how big

THE WITNESS: (cont'd.) that effect is. If the effect is very large, as the Dement study suggests it should be, the answer is sure, you would pick that up real easy. There it is, two fibers, you get a fivefold excess. I can pick that up easy. You would not even have to be a very good epidemiologist to pick that up.

But if Dement is wrong, say that I'm right, it's going to be hard to pick up a twenty percent excess.

Remember I said that perhaps epidemiology might pick up a one point five relative risk? That's like an SMR of one-fifty, or like a fifty percent excess.

I would say that epidemiology might pick up the excess if Peto/BACOA is right. It would be kind of borderline. You would have to have a pretty good study to do that. It's going to be very hard to pick up the excess if anybody else is right, who have lower slopes in their equations than Peto.

MR. LASKIN: Q. Why is that? Why aren't you going to be able to pick it up if the other estimates are right?

THE WITNESS: A. Because...

Q. You need too big a population?

A. Yeah. Let me show you the next slide. Okay, let's see the next slide.

You see, you would need a huge population and I don't even trust...I wouldn't even trust this, because since we are dealing with lung cancer and since lung cancer has many causes, and since it varies across social classes, economic groups, religious groups, and since it's highly sensitive to little differences in cigarette smoking, and since epidemiology is unable to control for all these factors, it's absurd to think you could pick up tiny differences without controlling for all these factors.

THE WITNESS: (cont'd.) Now, I've just expressed this in terms of how big would the population need to be to pick up an excess of the magnitude suggested by each of these six studies...six studies, approximately. Six studies, yeah.

Six studies - how large a population would you have to have? Well, conceive of this. Let's say if MacDonald were right, you would have to have a population of two hundred and twenty thousand people exposed at just two fibers, and that just simply doesn't exist. You would never find that in the world.

First, remember, I already showed you in the previous paper how big these populations are, and they are nothing like...you know, just to identify...you never could do it with the MacDonald or the Peto or the Enterline or the Wile.

Now, when you get down to the Peto/BACOA, you are getting close to something that might be doable. You might just be able to find four thousand people. That would be a big study though. Most studies are much smaller than that, that have been done on asbestos workers, and of course if Dement is right, it's going to be a piece of cake, no problem. If Dement is right, we'll pick this up real easy - easy to check on a two fiber standard.

MR. LASKIN: Q. To put this in perspective, Dement, I take it, has written a recent paper which is not published, but which has been promulgated by NIOSH?

THE WITNESS: A. Well, I don't know if it has been promulgated. Dement's paper has the same status, I suspect, as all the others on this list here, and we might spend some time talking about his paper, if you want to, later. But...

Q. It's the one we are probably not familiar with?

A. It's the one you haven't seen. It's a paper stamped, Do Not Quote, Do Not Publish, which everybody has

THE WITNESS: (cont'd.) and quotes, and it probably is the best way to get something quoted is to stamp it like that.

Q. I take it he has listed chrysotile asbestos amongst asbestos textile workers, is that it?

A. Right. He looked at a plant at Charleston, South Carolina, that used almost primarily chrysotile, and this in fact is his Ph.D. thesis at the University of North Carolina's Graduate School of Public Health...the School of Public Health, University of Carolina. This is his Ph.D. thesis, and so it has his name as an author as well as his advisers', apparently. I mean, the other authors on the paper turn out to be faculty members at the University of North Carolina, and...

Q. I take it your professional opinion from all of this is, that unless he is right or unless Peto is right, we are not going to be able to use the kind of cohort studies that people have been using in the past, to detect excess lung cancer risks from asbestos at a two fiber standard?

A. Right. I must say that what I ended up saying in this particular paper is that, all my calculations here are based upon detecting an excess in lung cancer. And I would say that if you were looking for an excess in asbestosis, it might even be tougher to pick it up.

Now, what I concluded was that unless Dement is right, if we are going to test the two fiber standard we are going to have to look for some end product other than lung cancer or asbestosis.

There are three possibilities. One is that you can look at x-ray changes, perhaps not representing clincial asbestosis, but simply detectable x-ray changes. Perhaps minor changes, one possibility. In other words, you could see do people exposed to two fibers have x-ray changes.

The second possibility is, do people exposed at two fibers have abnormalities in lung function, in breathing

THE WITNESS: (cont'd.) ability, particularly in pulmonary compliance or diffusion capacity.

I think that might be, that's a direction we really ought to move. One of the reasons I say that is, the x-ray changes that you can detect in populations with minimal exposures have very questionable significance in terms of disability, length of life, ability to function in any way. Whereas the kinds of things you can do with lung function tests do seem to be more directly related to ability to function, length of life, disability.

Now, the third possibility that has not been really looked into very well is, the possibility of doing something with the mesotheliomas. I think that mesotheliomas probably are produced in a subset of our population that for some reason seems to be susceptible. I think mesotheliomas are produced at fairly low fiber exposure levels, and this has not really been investigated very much, you know, what was the level of exposure that produced these mesothelioma cases.

But the fact that there are so many of these arising where there is only a casual kind of exposure to someone in the household, or living downwind of an asbestos plant, suggested the exposure must be very, very small for these mesothelioma cases. So the third way to go might be to explore the possibility of using the mesotheliomas to detect the effects of two fibers.

MR. LASKIN: Q. Can we go back to your table of differences? How does this Commission, sitting here, make some kind of assessment as to what explanation or explanations there may be for apparently widely divergent results that have been produced by various research studies?

THE WITNESS: A. Well, there are some very important methodological differences in these studies, and

THE WITNESS: (cont'd.) I think that those differences have to be appreciated and some attempt made to correct the differences among these various studies.

To give you an example, an important question when you do a study of, let's say workers in Toronto, is what will we compare these workers with. Now, Toronto probably has a higher death rate, let's say for lung cancer, than areas outside of Toronto, sort of characteristic of large cities is that they do tend to have higher cancer rates, and particularly lung cancer rates. We don't know why exactly, but this is a fact.

Now, would it be better to compare people from a factory in Toronto with other people in Toronto, or should we compare people from a factory in Toronto with other people in the province in Ontario?

Our government, for some reason, has taken the view that you shouldn't make a comparison with local rates, but only with national or regional rates. That makes a big difference.

Take, for example, the Dement study. It's in Charleston, South Carolina, which has a lung cancer rate greatly in excess of the State of South Carolina. That is, the city has an extremely high lung cancer rate. In fact, the city has an extremely high death rate from everything. Twenty years ago I wrote a paper about the death rate of coronary heart disease in Charleston....it's phenomenal, just unbelievable death rate from heart disease, and it led to a lot of epidemiological investigations - why should that be?

The point is that Dement made his comparison I think with national rates, and I could be corrected on that, but if he had compared with local rates, his excess would have been much less, much smaller. Well, this is sort of a battle of ideas. One argument is that perhaps the high death rate in Charleston is due to the fact that there was an asbestos factory

THE WITNESS: (cont'd.) there, and somehow that factory, asbestos products plant, influenced that rate and it would be unfair to use a local rate because it already had components of the problem already in it.

Of course the other argument could be that, is it possible that a little plant employing a few hundred people could influence the death rate for a community of how many thousand, I don't know, a very large area?

I personally think that the rate should be compared with local expectations, not national or regional expectations, and that the factors that may perhaps influence the high rate of lung cancer in Charleston are not related to the presence of this particular plant. So that...so why is Dement so high? One reason is they use national comparisons.

Why does he use national comparisons? Well, that's a policy of NIOSH to do that. They, just as a matter of policy, don't believe that local comparisons are justified.

MR. FINKELSTEIN: Q. Shouldn't that cancel out if he is doing a dose-response thing?

THE WITNESS: A. I beg your pardon?

MR. FINKELSTEIN: Q. Wouldn't that cancel out if he is doing a dose-response? It should be a cancelling background...?

THE WITNESS: A. Right. The slope wouldn't change, but one might...then if you used local rates you would find that some exposure groups have better expectations than the general population of the area, and then you might start raising a few questions. Then that might open up some other kinds of questions, once you find out, for example, that people exposed, let's say at one fiber, half a fiber, have a better mortality experience of lung cancer than the general population in the area, you might say well, gee, maybe there is some kind of selective factor at work here. I think then that opens up

THE WITNESS: (cont'd.) the door for asking a few other different kinds of questions about this whole thing.

MR. LASKIN: Q. I take it methodology isn't the complete explanation of the differences?

THE WITNESS: A. No. Let me say this, that the weakest part of all these studies, in my opinion, is the estimates of dose, and the one thing the Dement study has going for it is that Dement is basically an industrial hygienist, and I would think that he would have done a pretty good job on the dose side of the thing.

MR. LASKIN: Q. When you say the weakest part of all these studies is the estimate of dose, are you talking about a way of calculating dose or the past measurements, or both?

THE WITNESS: A. Both.

Q. Can we deal with each of those?

A. Okay.

Q. What's the difficulty with the way people have calculated dosage?

A. Remember how we calculated dose. Dose was the average exposure multiplied by time. So dose has two components. One is the thing you can control, we legislate the average exposure, intensity, or dose rate, as it's called. The other factor we don't pay too much attention to. That is, how long did somebody work. Certainly in the field of occupational health we haven't paid much attention to how long should somebody be allowed to work with something.

I think in radiation we have, perhaps, but it hasn't really...an idea hasn't really caught on in other areas.

Okay, there is a fallacy in calculating dose that way. Think of it this way. What if we...let's forget about the intensity part of it. Let's just take the length of time part of dose...because after all, it makes about half the contribution.

THE WITNESS: (cont'd.) Remember, the dose is intensity times time. So half of our measure, we end up with that number one hundred I had down there, half of that was time, fifty years, and half of it was intensity, two fibers.

Okay. Let's take away the intensity and think about what we've got. How could you live a long time? Well, let's put it the other way. How could you get a high dose? What if time is the meausure, how could you get a high dose? You would have to live a long time to get a high dose.

After all, if dose is simply duration, the only way to have a high duration is not to die. Now, that's nonsense. I mean, how can you have a dose measure that contains elements of both the outcome measure - dying - and the input measure - dose. That's nonsense, and I think there's a fallacy in all these studies that tend to let the followup period and the dose period be..they are both time dependent and they are running together.

Once again, you cannot get a big dose unless you live a long time, and living a long time is incompatible with having a high death rate from, say lung cancer.

Studies I've done...here's a problem, and I've written about it, and the way I've avoided this was to study a retired population. Now, why would that, what's nice about that?

Well, if you were to do...let's back off and see, what would we like to know? What we would like to know is...if we were to do an animal experiment, let's say, how would we do it?

Well, we would dose some animals, terminate the dose and wait and see what happened. We would stress them with something and see what happened. I mean, almost all of the experimental work is done like this. You stress the organism and you see what happens.

Now, what's the analogy with a retired population?

THE WITNESS: (cont'd.) What in effect you've got there is, you've got people who had a lifetime worth of exposure, and then the exposure just terminated. The termination has nothing to do with the individual...the individual does not choose that termination. Society says at age sixty-five you are retired, and most everybody in the work force does that. In the United States, sixty-two, they get out.

So what exists there when you study that situation, in other words, how you do it, well, you look at the dose that occured during the sixty...first sixty-five years, and then you look at the response that occurs after the dose terminates, and followup starts.

You start the followup at the termination of dose.

Now these other people, these studies, haven't done that. The followup period and the dose accumulation period run concurrently in all of these studies.

What is dose? Dose is the duration of exposure. How do you get a big exposure? Don't die.

So there are two ways to do studies. One is, don't start the study until the dose is terminated, and the other is, let the dose and the followup periods run concurrently.

I think there is a fallacy in doing it the way many people do it. In my analyses of data recently I have sort of set up a pseudo-analysis where I only count the dose up to some period, and then only count response after that period. Recognizing that I have no way of controlling dose after I start my followup, I simply just ignore it.

That's kind of a compromise way of doing it.

That's kind of a long answer, but I think that there are a lot of differences among the studies, that we haven't even dealt with. They are really all done in somewhat different ways, with different concepts as to how you deal with this problem of dose and followup proceeding concurrently

THE WITNESS: (cont'd.) through time.

MR. LASKIN: Q. If you do that, it would tend to overestimate the relative risk, in your view?

THE WITNESS: A. I really don't know, you know. I think that...I don't know.

Q. What about the intensity of dose? Is there a difference between a person who gets short bursts of very intense doses, as opposed to the same amount of dose over a longer period of time?

A. There appears to be. In the studies we did of the retired workers...incidentally, this is the study of the observation on the entire population of the Johns-Manville Corporation, and in that study we found that the people who had high intermittent exposures...such as maintenance people...seemed to have a disproportionate amount of cancer. Now, it's true that we have a hard time estimating dose for somebody who, let's say, fixes something every other day and at that time he gets a hundred fibers per c.c. for fifteen minutes, or something like that. It's a little hard to conceive of how you calculate an average dose for that sort of a situation. In fact, the whole notion of time weighted averages doesn't fit that situation very well, you know, but we did...the best estimate we could make suggested that those high intermittent doses apparently were having a disproportionately large effect on the incidence of lung cancer.

Two other bits of information sort of suggest that. One is that there is a theory of overload in biologic systems... it comes largely from chemicals...that you have different metabolic pathways, but you can overload those pathways.

But it also works with dust diseases. There is the notion that you can overload the lung, there is a clearance mechanism. It's designed for some capacity that you could...if you intermittently overload that capacity, the consequences...

THE WITNESS: (cont'd.) there are some consequences of overloading the clearance capacity.

The second thing is, that it's about the only way that I can explain the very high cancer incidence that you observe in insulation workers. The...

MR. LASKIN: Q. Dr. Selikoff's work?

THE WITNESS: A. Selikoff's work. The estimates they make of the dose suggest that they should have a much lower excess in lung cancer than they actually observe, and I think in one of my slides I showed that, that they had a twenty-two percent excess and most everybody else has a fourteen or fifteen percent excess at a given dose, and you know...a lot of explanations just seem to fit the facts and I suspect they are kind of generated to make the facts seem reasonable.

But I think there is something in this. I think high intermittent doses probably have disproportionately large effects on disease.

Q. Are you suggesting that insulation workers may be a class of asbestos workers that are given high but intermittent doses?

A. Right. I think perhaps one of the reasons for the very large excess in insulation workers is the way they get the dose.

In a factory, in a textile factory, the dose is fairly constant. You can pretty much predict what it's going to be from minute to minute, hour to hour. The machines run at a constant rate, the amount of materials processed is constant. It's a very different situation than somebody who tears a pipe down, or something falls on his head and he gets a big blast of something, and then perhaps leaves the room and does something else for four or five hours and has no dose at all.

I suppose intuitively you would think the

THE WITNESS: (cont'd.) consequences must be somewhat different for these two very different situations.

MR. LASKIN: Q. Would the intensity of dose have any effect on the latency period?

THE WITNESS: A. I think so. And I think recently some evidence from one of Irving Selikoff's studies tends to support that. It certainly is true in animal models with chemicals, that you can vary the time to tumour by varying the dose rate or intensity, as you put it.

Q. What's the effect?

A. Well, the idea is...

Q. Is there a dose-response relationship?

A. Yeah...well, it goes back to 1967, when there was a paper written by a German named Druckrey, who looked at this in animals, showed that this in fact was true, and then more recently a paper by Jones and Grendon formulated the relationship that the time to tumour is...the time to tumour is proportional to the inverse cube root of dose.

So right - T is equal to one over...okay, T is equal to one over the cube root of D. So it's the...the time to tumour is one over...is the inverse of the cube root of dose. T is equal to one over the cube root of D.

Jones and Grendon reviewed many, many studies and this is their formulation. People say, well, it ought to be the fourth root or square root, but I think in asbestos one of the reasons you might believe that is that the fibers tend to stay in the lung, and there is a notion called dwell time that most people haven't tried to deal with, but perhaps a very small dose at a very young age might, after many, many, many years have an effect, because the fiber was there for a very long time.

Q. If you apply that principle then, how small

Q. (cont'd.) does the dose have to be before the latency period exceeds the life expectancy period?

A. Well...

Q. Can you come to some conclusion on that?

A. Yeah. I worked on that and...it looked to me, the way this works out is like this: If you extrapolate from the animal work on this...in other words, you calculate this from the animal models...it appears that in dose ranges starting around four fibers, most of these cancers are going to appear in man's lifetime, and so that invoking this Druckery notion, he sort of gets credit for this, doesn't make much difference.

But where it does make a difference is when you get down to very tiny doses. Then the mean time to tumour might be a hundred and fifty years, or something like that, and I've got a paper on this...you haven't seen it...but...so what you get if you draw these curves is, you would see that in the dose ranges I've been talking about, the ones on these graphs, most of these tumours, the time to tumour is short enough that they are going to occur in man's lifetime in the occupational environment, which means that we can't really, by looking at doses of two fibers, four fibers and ten fibers, see the Druckery phenomenon.

But if you just simply take his calculations, you begin to see that if you get down to a tenth of a fibre, those are occurring mostly beyond man's lifetime.

Now, the frustrating thing is that this is all not observable. That is, we can't do studies of a tenth of a fiber for the reasons I've just said. You would need a population bigger than we have.

So, I don't know if we can ever resolve this issue. It's going to be a hard issue to resolve and I think there will continue to be some debate. I would guess the debate has gone against Druckery in the last few years, and I haven't kept on it, you know, but it's an idea. What we have

THE WITNESS: (cont'd.) are battles of ideas here, concepts and ideas, and you know, some ideas get popular and then they kind of fall aside and new ideas take their place.

MR. LASKIN: Q. Is what you are suggesting then that the shape of the dose-response curve, at least at some portion, is not going to be linear?

THE WITNESS: A. At the lower levels, if Druckery is right, it will be a nonlinear dose-response. It would look like the one I showed you in the upper righthand corner of that thing. It would be a power function of some kind.

But it would flatten out pretty quick. What you are getting, you see, is these tumours moving into man's lifetime. As dose goes up, they gradually move into man's lifetime and you get all of the response in his lifetime and you get a nice flat curve. As the dose gets smaller, they start to move out of his lifetime, and you start to get this parabole effect at the bottom end of the curve.

Q. Can we go to the second problem with respect to dose, that you dealt with, and that is the measurement problem? With respect to historical cohort studies, I take it.

A. I think the best example of that is what happened to the Peto estimates. I showed on one of my slides two different regression coefficients for Peto, and the story there is that the method of fiber counting that the British used tended to greatly underestimate the...if you compare the British method with the American method, they were greatly underestimating fibers, historically. They seemed to be counting fibers, they were reporting everything in terms of fibers, but they were using a different method to the Americans.

We use something called the graticule counting method, and they used a different method.

Peto went ahead and published papers based upon these underestimated counts...the BACOA paper, the Simpson

THE WITNESS: (cont'd.) report, for example, is based on underestimated counts. If you follow the literature a little bit, you find out that suddenly the industry came along and said, look, we really said there were whatever...and there were four or five times as much dust in the atmosphere of our plants. We were using the wrong method. If we use the graticule method, the American method, we were off by factor four or five.

So I'm sure Julian Peto must be frustrated as the devil about this whole thing. He thinks that they are shifting ground from under him. I think it's probably an honest difference in the way you count things.

But I think there's a lot of bad communication in the literature because of different ways of counting things, and lot of it hasn't even surfaced. This only surfaced in the last couple of years, this business about the British counting method. We always thought the British were great. They always reported things in fibers. Gee, isn't it great, it took Americans years to get around to where the British were, and now we find out the British were using..you know, their fiber counts weren't even comparable with the American method.

DR.UFFEN: Is there not any international comparison between laboratores? Standardization?

THE WITNESS: A. I don't know. I know there is among laboratories within the United States, but I don't know. I think...and I would suggest that this whole area, you might talk to people that are in the business. I am only peripherally in the industrial hygiene business, but some of the people you are going to have speak to you will know more about this than I do. But, you know, the other problem, of course, is how do you estimate exposures during periods when nobody was counting anything?

MR. LASKIN: Q. How confident can one be about exposure estimates?

THE WITNESS: (cont'd.) I think you have to have a lot of confidence in the people who are making them. My estimates on the Johns-Manville Corporation were made by somebody who I think probably at the time was the most knowledgeable guy in the world on this thing, the industrial hygienist for Johns-Manville.

Not only that, but he did have...we did have dust counts. We did have about ten years worth of dust counts...where they measured. Remember, my studies...one of the early studies published in 1973, we were estimating dust exposures in 1969, so we had exposures back to 1959, ten years back from then, and then you do know a lot of things about a factory. You know when ventilation equipment was installed, you know the amount of air that it moves, you know the design characteristics, you know the rate at which the machinery operates, you know the rate at which the looms are operating, you know if the looms were running slower, or spinning was running more slowly in 1930 than in 1940 - this would affect the amount of dust.

That is a lot of parameters that are known, that help in making dust estimates.

Now, how fully the people making the estimates take these parameters into account must vary a lot from place to place. I think there is an optimum way of accounting for all these things, and I have some faith, I think, in the Johns-Manville estimates that I have. I have ways of checking on these, and I think that in fact they are probably not far off.

Now, the problem, of course, is these are all particle counts. The historic way was to count particles, not fibers. The big question is, how do you go from particles to fibers.

MR. LASKIN: Q. How do you go from particles to fibers?

THE WITNESS: A. I don't know how you, you know, again, when you get some experts on that, you ask them about that.

My attitude has been that the particle counts we had were highly predictive of the incidence of lung cancer, and I think even...MacDonald went so far as to say that maybe what we should be controlling are the particles because they are so predictive.

I guess that just kind of says, well, we must have been measuring the right thing, or about the right thing, or we wouldn't have had this great dose-response relationship from this whole thing. That's kind of a backhanded way of saying maybe they are not so bad, but I think this is totally unknowable, how good these estimates really are.

MR. LASKIN: Q. Have you tried to convert your own measurements to fibers?

THE WITNESS: A. I never did until I did these last couple of papers, and I realized that what I was talking about was incomprehensible. Nobody was interested in particles per cubic foot. By that time, people that I had a great deal of respect for had come up with conversion factors and I said, well, you know, they are pretty smart guys, maybe they are right, I'll just convert.

Q. What's the conversion factor?

A. I was using three. A million particles equals three fibers per cubic centimeter.

Q. There's one other matter you mentioned in a number of your articles, and that's what you seem to call this multiplier effect that asbestos has. Can you help us a little bit with that?

A. Yeah. I think asbestos has a very unique property, perhaps unique property, and that is that it seems to multiply all the other carcinogenic effects that exist in a population. I think the consequence of that, perhaps, explains

A. (cont'd.) a few things about lung cancer and asbestos. People ask, why didn't we notice the lung cancer in asbestos workers, sooner. Well, first of all you have to realize that lung cancer in the 1930's was a rare disease....a very uncommon disease.

Now, suppose that asbestos does multiply other effects. Suppose that all...let's say that in 1930 there was an incidence of lung cancer that had causes, whatever they were we don't know, and let's suppose that asbestos had its same multiplication effect that it has today, it multiplied the effects of those causes. It may be a factor three or a factor four, or whatever, but if you take a rare disease and multiply it by a factor three or four, you still have a pretty rare disease. Nobody is going to notice it very much. If you multiplied angiocarcomas by ten or twenty, you still wouldn't notice them. It's very rare. Mesotheliomas are the same way.

The thing that tipped me off to this was the fact that in my studies of retirees from the Johns-Manville Corporation, the probability of their getting cancer depended mostly on when they retired. That was very strange. Why should a guy retiring in 1941, who must have had terrible exposure, have a much less risk, absolute risk, of getting cancer than a guy retiring in 1960?

When I calculated it, I found out that the guy retiring in 1941 had the same relative risk...remember the whole population was very low in 1941, so you multiplied that low factor by three...the same relative risk, but the absolute risk, the increment, was very tiny because he was multiplying a small number by a factor, so the absolute increment isn't very big when you multiply a small number by something. If you multiply a big number by the same factor, the absolute increment gets to be very big. You can plot that out and see if it works.

THE WITNESS: (cont'd.) So when I looked at it through time, I noticed that the relative risk was the same in 1941 or 1951 or 1961, no matter when he retired, but the absolute risk was very, very different.

I looked at some other data, Selikoff's data, it's the same thing as that data, and now if you add the smoking situation to that you find out that what asbestos is doing is simply multiplying the existing risks by some constant. That's a curious situation.

So, that makes this thing very important that I brought up earlier...that is that you better know what the background rate is when you calculate the increment caused by asbestos in a particular situation. I think that's a reason, for example, that Selikoff's study of insulators in the New York City area produces a much higher relative risk, using the U.S. population as a standard, when compared to a study of insulators throughout the entire United States. Now there, of course, the U.S. standard would be appropriate.

You know, one might well ask, why do the insulators in New York have a seven or eightfold relative risk of lung cancer, and the insulators in the whole U.S. have a four to five excess relative risk? They are doing the same job, what's the difference?

Well, the difference is, one group lives in an area that has a high indigenous lung cancer rate, and in calculating the relative risk for that group in New York City he should have used the New York City rate.

In one of my papers I did that, and I show that I can account for maybe half of the variation among studies by simply changing the base upon which you calculate the expected number of deaths. And I think that all is tied up with this curious property of asbestos that seems to somehow just multiply existing risks by some constant...maybe four, maybe five.

MR. LASKIN: I think, Mr. Chairman, to be fair to my colleagues I should turn Dr. Enterline over to one of them.

DR. DUPRE: Thank you very much, Dr. Enterline. Do your colleagues wish to take a moment to decide who wishes to have the honour of going first?

MR. LASKIN: Just a second.

DR. DUPRE: Dr. Enterline, do you wish to take a quick break in the meantime?

THE WITNESS: A. No, it's all right.

MR. LASKIN: Would the Commission like five minutes?

THE WITNESS: It's up to you.

MR. LASKIN: Well, why don't we...I suggest we take five minutes.

DR. DUPRE: Why don't we stretch until noon.

THE INQUIRY RECESSED

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THE INQUIRY RESUMED

DR. DUPRE: Have you gentlemen agreed on an order?

(REPORTER'S NOTE: There is general discussion here as to the order.)

MR. LASKIN: Mr. Chairman, may I just go into evidence just formally these two additional papers.

DR. DUPRE: Thank you.

MR. LASKIN: Let me mark it exhibit two so everybody will be aware of it. First of all, the paper, Proportion of Cancer Due to Exposure to Asbestos will be exhibit two.

<u>EXHIBIT # 2:</u> The abovementioned document was then produced and marked.

MR. LASKIN: Exhibit three will be, Epidemiologic Basis for Asbestos Standard, and what I might also do, Mr. Chairman, because Mr. Dement's paper was referred to and I have a copy of it and I take it it's available to be put in, and I'll distribute it to the parties, let's mark Mr. Dement's paper, which is called Estimates of Dose-Response for Respiratory Cancer Among Chrysotile Asbestos Textile Workers, let's call that exhibit four, and I'll distribute that to the parties over the lunch break.

EXHIBIT # 3: The abovementioned document was then produced and marked.

EXHIBIT # 4: The abovementioned document was then produced and marked.

DR. DUPRE: Mr. McNamee?

MR. McNAMEE: Thank you, Doctor. I have no questions. I think I indicated to my friend that I didn't have any.

DR. DUPRE: Thank you.

Is it Mr. Starkman or Miss Jolley who wishes to go next? Are you...Mr. Warren?

MR. WARREN: Yes, I think I'll go next.

DR. DUPRE: If you please, Mr. Warren.

CROSS-EXAMINATION BY MR. WARREN

Q. Dr. Enterline, why don't we go back to your original paper in 197...I guess it's 1972, for a minute, so that we can understand basically where you come from in this business.

The paper I am referring to is, The Mortality Relation to Occupational Exposure in the Asbestos Industry, and I believe it is number three on this list that Mr. Laskin

Q. (cont'd.) provided to us earlier. Now, what I would like to do in the beginning here is simply put your historical exposure in your cohort into some perspective so we know what we are comparing it with today in industry.

Now, if I look at table four on page 899 of that study, your average mean particle counts are two hundred and thirty for the production workers, and two hundred and thirty-six for the maintenance workers. Are you with me?

A. Yes. Table three?

Q. Table four.

MR. LASKIN: Table four.

MR. WARREN: Q. Table four. I'm sure that I said table three. I was mistaken.

THE WITNESS: A. Okay, yes.

Q. Now, if we can translate those particles of yours to fibers, how many fiber years, in your estimation, are we talking about for the workers in the original study?

A. Well, you have to multiply by three.

Q. You think three should be the number?

A. Oh, I don't know. That's what some people think. I think it depends on the operation. I think in asbestos cement, maybe that's the wrong number. I think perhaps in textiles it might be the right number. I don't know how anybody comes up with a number like that anyhow.

But I've done it, yes. I've made the translation.

So you multiply that all by three and you get fiber years.

Q. Okay. So if we multiply by three, we are talking about, say six hundred and ninety...say seven hundred fiber years, roughly, that on the average your cohort was exposed to, correct?

A. Yes.

Q. Now, let's talk about a person today exposed

Q. (cont'd.) in industry under the two fiber standard. First of all, if we have a two fiber standard would we expect that workers on the average would be exposed to two fibers, or to something less than two fibers?

A. Well, if industry observed the two fiber standard, yeah, you would less than two fibers, sure. I mean, I think you've got to be realistic about this though. If you have a two fiber standard, what are you really going to run at.

My guess is that you are going to get some people at four and some at one, or something. But I think, right, if you truly observed it, then the average would be something less than two.

Q. Do you have any guess as to what you have to run a factory at in order to stay within two, on the average?

A. No. I have no idea.

Q. Okay. But we can say that the numbers, on the average, in a factory that is observing a two fiber standard should be less than two?

A. Right.

Q. If we have a worker who is exposed...first of all, maybe you can tell me this from your cohort. What was the average number of years that a worker was employed in an asbestos plant? Do you know that?

A. Yeah. On average, twenty-five years. At the time of retirement the average working life had been twenty-five years, in the asbestos industry.

Q. So that if your average in your cohort were to apply to the man in my two fiber standard asbestos plant, and he worked for twenty-five years at two fibers, that would be fifty fiber years?

A. Right.

Q. And if the plant was being operated in such a way as to make two fibers, then on the average we would have

Q. (cont'd.) that man exposed to less than fifty fiber years?

A. Right.

Q. So, just to keep everything in perspective, what we are talking about with your cohorts are people who had exposure ten to fifteen times the...what my present hypothetical man would have in a two fiber standard plant?

A. Right. I think they are probably...you know, be specific, I think they must have been exposed to an average of about thirty fibers.

The average particle count was, if you calculate it for these workers, these people, was ten million particles per cubic foot, on average. If you translate that to fibers, that would be thirty fibers per c.c. That's pretty high by current standards.

Q. Something that Mr. Laskin asked earlier about was why you received such a large elevated risk for your maintenance workers as opposed to your production workers.

A. Yes.

Q. I think you attributed that to high intermittent exposures?

A. That's an explanation, right.

Q. I was wondering, when you were speaking earlier about mesothelioma and you were talking about the exposure of persons in the home, is it likely or possible that the excess incidence of mesothelioma that has been observed in some nonoccupational settings, could that be attributed likewise to high intermittent exposures? For instance, the woman shaking out her husband's clothes when he comes home at night, and washing them?

A. It might be. I just don't know of any evidence...

Q. One way or the other?

A. ...one way or the other on that, right.

Q. What do you have in your study with respect to this question of intermittent exposures? Any data to suggest how much higher than the average such intermittent exposures might have been? In other words, do we have any sense of portion when we are talking about intermittent exposure? Are we talking about exposures that are five times the average or ten times the average? What's our dimension here?

A. I don't know. I think...I don't know how high it would go. I don't know if there is a physical limit to how much fiber an atmosphere can contain. Perhaps there is. But I would think that maintenance workers, for example, who are working with a ventilation system that's out of order could get some extremely high exposures for very short periods of time, and I don't know what that is. It might be a hundred fibers, two hundred fibers, something like that.

Q. So this would have been men who would have gone in to clean up the operation and would have been in the ventilation equipment tinkering around with it, trying to fix it, for several hours perhaps?

A. Sure. I would think so. Yes.

Q. When we look at your cohort, you've...I know you tried to get some idea where in your own cohort you are unable to see an excess incidence of lung cancer, is that right?

A. Where I'm unable to see an excess?

Q. Unable to detect it?

A. Unable to detect it?

Q. Yes.

A. Well...

Q. I guess I'm...let me refer to what I'm discussing, and that is a paper called, Respiratory Cancer in Relation to Occupational Exposure. This is the one published

Q. (cont'd.) in the British Journal of Industrial Medicine in 1973. Let me see if I can find that. That's number four.

A. Okay. Oh, I know what you mean, yes. You mean a statement I have about large increments appear to occur at...?

Q. Yes. I think what you've done, if I understand it, is to try to see where you are not observing an excess incidence, at what average fiber, average particle counts you are not seeing an excess.

A. I don't think I did that, as I recall.

Q. Well, explain to me exactly what you did then?

A. What exactly are you referring to?

Q. I'm referring to the material on page 165.

A. 165...I think the only statement I can recall, and maybe you can pick out a different one, is that at a certain level there appears to be some large increments, which I think if you look at 165, it certainly looks that way. Something happens...in other words, below two hundred, everything is just going around in circles in those dots. You can't make much out of that. But all of a sudden, you get above two hundred, it takes off. I think I have a little discussion of that phenomena, although I must point out that even at zero I had an excess in that particular...and we are still getting some kinds of responses down in that area. That could be just noise. It could be bad comparison groups, or something like that.

Q. Why don't we take a look at...if we look on table two of this study, at a hundred ...and I'm on the previous page, 164...what we see for dust exposure below a hundred and twenty-five is an SMR of one sixty-six point seven, right?

A. Well, I can't...mine is erased. I guess that's right. I can't tell. That margin on my paper is gone.

A. (cont'd.) So I can't tell, but I think that might be right though.

Q. Yeah, if you look at your paragraph on the righthand column right above that table, I think it says that in the text as well.

A. Yeah, I think it's about right. "Of particular interest to the SMR, one sixty-six point seven?

Q. Yes.

A. It's less than...all right...mmm-hmm.

Q. Those are for the people below a hundred and twenty-five?

A. Right.

Q. Now, this SMR is, I think, as you suggested previously, an SMR calculated by comparison to the general population?

A. Yes. Mmm-hmm.

Q. Based on what you said previously, in your view an appropriate and maybe the best way to develop an SMR, would be by comparison to similar urban populations?

A. Yes.

Q. Have you made any adjustments with respect to urban populations to recalculate an SMR for these workers?

A. I did recalculate an overall SMR that appears in a subsequent paper, the Pitfalls in Epidemiological Research paper, and it didn't make very much difference. I never recalculated for the subgroup of five hundred thirty-three. Although I don't think it would matter. I think the location parameter is the one that's important here...that is, where is the plant located. It looks as though the locations of these plants, the areas the plants are located, that the background rates were just about like U.S. rates.

Q. Let me turn to the next page, because from what you are saying I am getting confused now. If you look in

Q. (Cont'd.) that first column on page 165, and I think we are still talking about the same group of workers...

A. Yes.

Q. ...it says, "One of the factors that probably elevates somewhat this SMR is the fact that men in the study were mainly urban dwellers and respiratory cancer death rates are normally higher in urban than in nonurban areas. Thus since the expected number of deaths is based on the entire population, it may be too low. Correction for this for men with total asbestos exposure under a hundred and twenty-five reduces the SMR for this group to a hundred and forty-six point two."

A. I forgot I did that. Right, okay. Okay, it must be right then.

Yeah, okay. I did do that.

Q. So that if we do make that adjustment in your cohort, we are down to a hundred and forty-six point two as the SMR for that group of workers?

A. Yes.

Q. Can you tell me what...if you will take a look at the next paragraph here and can you explain to me what you were doing in the adjustments which you made in the next paragraph, because frankly, I'm a little bit lost?

A. Yeah, I haven't read this for ten years, you know.

Q. I understand.

A. My God, I can't...did I ever say that? I don't know, where did I get...I don't know who these people were.

Let's see, "A manufacturer of building materials is exposed to some dust contained..." I can't remember. I'll have to look it up. I don't know where I got that.

A. (cont'd.) I had the whole Johns-Manville retired population. Okay...I did have other people that I knew about. There must be another subcohort that I have forgotten about that I had that I never really reported on because there was no asbestos in it.

That's an interesting statement. I'm sorry. I just can't remember what that was.

Q. Let me see if I can reconstruct it from what I understand here. It appears that you have another subcohort of unexposed workers, and that you have sufficient data from this three hundred and twenty-four workers to calculate an SMR.

A. Right.

Q. And that SMR is one hundred and sixty-nine point eight.

A. Yep. That's what it sounds like.

Q. So that for a group of unexposed workers here, you are getting an SMR of one sixty-nine point eight, which is really very close to the SMR which you calculated for your asbestos workers exposed below a hundred and twenty-five particles?

A. That's right. So I, you know...obviously my intention there was to see whether there was some kind of a selection bias here. I was obviously concerned about the fact that the Y intercept here wasn't a hundred, and I wondered if the whole curve might be shifted up somewhat.

Not very big numbers, but let's see...okay, very astute. I forgot that was in there.

Q. All right. Not very big numbers, but basically about the same numbers we are talking about when we calculate your one sixty-six point seven SMR, too. If you look on the previous page, that's fifteen observed at nine...

A. I have fifteen here at nine, but...

Q. So if I understand what we are talking about

Q. (cont'd.) here for...

A. Nine...yeah, right.

Q. So that if I understand what we are talking about here, for your asbestos workers exposed below a hundred and twenty-five particles, you calculate an SMR of one sixty-six point seven, and for a group of unexposed workers, which are referred to in this paragraph on page sixty-five, you calculate an SMR of one sixty-nine point eight?

A. Right.

Q. So that when we are talking about your cohort below a hundred and twenty-five particles, we look like we are getting very close to the background incidence that you are getting among unexposed workers?

A. Yeah, it does. Right.

Q. Right. Now, just to keep that one in perspective, when we are talking about a hundred and twenty-five particle years, we are talking about three hundred and seventy-five fiber years? Correct?

A. Right.

Q. Once again, bringing that back to my worker presently exposed for twenty-five years to two fibers, he has fifty fiber years?

A. Yeah.

Q. Okay. Now ...

A. Of course you don't know what the average exposure was. Do I have that anyplace?

Q. You don't know, that's a good point.

A. No, I do, I do. On table two I show the mean exposure is sixty-two point nine...page 164. So the average exposure in years is sixty-two particle years. Okay, that's right.

Q. Let's talk a little bit about this high intermittent exposure situation once again. As you went over

Q. (cont'd.) it earlier, one of the things that you mentioned in response to a question from Mr. Laskin was that perhaps an explanation for the greater effects seen from high intermittent exposures might be that bodily defence mechanisms are overwhelmed when you get a large exposure. Can you explain to me what that means in the context of asbestos, or what it may mean in the context of asbestos?

A. I really can't...you know, you are getting out of my field. I am only quoting...I can't do that, I can't do that. I'm only quoting what other people have said and physiologically I simply couldn't give you a rational explanation of that.

I can talk about it in terms of chemicals that I know more about, but you know, it has to do with the rate at which the lung clearance mechanism functions and I just have to defer to somebody who knows more about that than I do.

Q. Let's talk in the chemical context for a second, so that we can understand the context, and then can you explain to me how it is possible in the context of chemicals that a high exposure can have a disproportionately greater effect than a low exposure?

A. Yeah. In chemicals there is some support for a hockey stick dose-response curve. In other words, two slopes - a slope up to some level, and then a take off and a new slope - it looks like a hockey stick. Take for example styrene. Styrene is metabolized up to some level, I can't recall the level...I can't remember what it is. Anyhow, I know that it is metabolized. We have done some experiments in our department with this. It is metabolized up to some level and after that dose rate is exceeded, it is simply stored as pure styrene in fatty tissues.

It appears that as long as the body is able to metabolize styrene, and I think the metabolite styrene oxide, it

A. (cont'd.) effectively deactivates the carcinogenic properties, effects, and you do get some...apparently because of differences, among individuals that show up in their ability to metabolize, but you do get kind of a shallow slope.

But there is a point where most of us are unable to any longer metabolize. The rate becomes...exceeds the capability of the metabolic mechanism, at which time we simply store it...apparently as pure styrene, at which time you get a different slope.

Now, whether there is any analogy between that and asbestos, I simply don't know. But I think this is pretty well established, perhaps for chemicals in addition to styrene.

Q. Okay. So keeping with chemicals first, if this biological hypothesis of overwhelming defence mechanisms applies in the case of chemical carcinogens, putting aside asbestos, the shape of our dose-response curve is going to flatten out, is going to show an inflection at the lower dose levels, correct?

A. Well, if you can make the analogy, I am not sure...we certainly don't metabolize asbestos.

Q. I'm trying to keep away from asbestos first of all in that question.

A. Okay.

Q. In other words, if we are talking about chemicals, let's say styrene which you discussed, for chemicals the shape of the dose-response curve will flatten out, there will be an inflection at lower doses as compared to higher doses, if this..

A. It depends what the metabolite is. Very often the carcinogen is the metabolite and not what you are taking in, so, you know, you talk...you will have to talk about a class of chemicals for which the metabolite is not...has no carcinogenic properties. I simply don't know what's in that class. I suspect

A. (cont'd.) there is such a class of chemicals, however.

Of course, many carcinogens really are produced by the body, metabolized into carcinogens, so you have two kinds going here. But, okay.

Q. But let's take, just for purposes of understanding the model first of all, let's assume we are talking about a carcinogen which itself...a substance, a chemical...which is itself a direct carcinogen and not the metabolite of that substance.

A. Okay.

Q. And let's assume once again that the defence mechanism hypothesis which you are talking about is in fact true. Then, from a statistical standpoint and only a statistical standpoint, the shape of our dose-response curve is going to be one that is going to tail off and flatten off as doses go on to low levels?

A. Yeah, right. That's true.

Q. Now, recognizing that you don't feel qualified to discuss from a biological standpoint or a physiological standpoint how such defence mechanisms might operate in the case of asbestos, is it nonethless true that your observations of a higher incidence of lung cancer for intermittent doses, is that observation consistent with a bodily defence mechanism theory?

A. Yeah, right. I think the theory is a kind of explanation for the empiric observation. But it supports the theory that high dose rates have a disproportionate effect on, in this case cancer incidence, if you go back to the asbestos problem.

Q. So that once again, not asking you to be a lung function expert and not asking you to be an expert in the biological mechanism itself, the data which you report are

Q. (cont'd.) consistent with that theory?

A. Right. I think so.

Q. Now, another thing we discussed earlier which might affect the shape of the dose-response curve is the...let me back up one step. When we were discussing your observations about intermittent exposures, I believe you said that you felt your observation was borne out by Dr. Selikoff's insulation study. Can you explain why that is?

A. I simply thought that the only way I could explain the very high response he got to what appeared to be a low average exposure, was in terms of the way in which the dose was received. And I thought that his insulation workers might be very comparable to my maintenance workers. My guess is that they must have gotten doses in about the same way, and so kind of by analogy I say, then maybe I can understand...in other words, I should not compare my overall findings with Dr. Selikoff's findings, but perhaps only my maintenance worker findings. They are closer, certainly, to his findings.

So I think that...if I am true, if this is right, that high doses, high intermittent doses have a disproportionate effect, then I think perhaps we better understand the whole picture that we see from these various epidemiologic studies coming up.

Q. In other words, without that hypothesis it would be harder to understand the SMR's which Dr. Selikoff observed?

A. Right. I would have a hard time understanding that, given that the...remember now, my doses are averaging about thirty fibers, and his doses perhaps are eight or twelve or something like that. That is, his time-weighted average dose. To come back to these series of dose-response curves, these slopes I showed, how can one explain those kinds of differences? There must be other variables that we have just

A. (cont'd.) not measured in this equation that need to be taken account of.

Q. You bring up a point there that maybe we ought to talk about for a second. Did Dr. Selikoff reconstruct the doses to which his workers were exposed in a manner comparable to the way you attempted to reconstruct the doses to which your workers were exposed?

A. I have no idea. When you get Bill Nicholson here, ask him. He did it. He'll tell you.

I know that in the early days they were very reluctant to discuss fiber counts or fiber levels, and it might be because it was so difficult to guess what they might be...given that the insulators didn't always work with asbestos, you know, they weren't always using that as an insulation material or even tearing it out. There must have been other things. It was kind of downstream aways, kind of late in the game when they finally came up with some kinds of fiber estimates...counts... estimates. And there is a good...once again, when Bill Nicholson comes, you ask him about that, he's got a couple of good papers on that. He probably can tell you how they did it.

Q. Okay. When we are discussing the shape of the dose-response curve this morning, one thing you mentioned was the possible inverse relationship between dose and latency period. I believe you quoted others as saying it might be inversely related to the cube of dose?

A. Right, okay. Cubular dose, right.

Q. Now, I think also you suggested that there was a fiber dose at which that relationship might begin to show up, but that previous historical cohort exposures were too high for that inverse relationship to appear. Is that correct?

A. Yes, that's right.

Q. Have you made any calculations as to where... kind of comparable to the calculations you made this morning...

Q. (cont'd.) or maybe it's not comparable, but have you made any calculations, let's put it that way, about where that relationship would begin to show up?

A. I think I mentioned that certainly below the historic exposures that we know about, and I don't think I did. My intuitive feeling is that it might be at or below a fiber that you might begin to see that. If you had large enough populations with very low exposures, like point five or something, you might begin to see this effect.

Q. I think you, in response to a question from Mr. Laskin on this, you said that if that relationship is in fact true, that it likewise would cause that dose-response curve to tail off at low doses?

A. That's right. As far as we are concerned with finite lifespans, it would be a par function, it would be a...let me, you know, it looks like...we are talking about this sort of thing...I guess this is drawn or something...

MS. JOLLEY: Yes.

THE WITNESS: A. But I think that you have seen dose-response curves...I don't know, that are somewhere...we've been looking at something like this, and I don't know what is happening, what's going on down in here. Perhaps if we get way down in here you might start to get the feeling of something like that.

So I think when we look at occupational sort of thing, we are getting a picture, a window up in here someplace. But the unknowable is this part, for the reasons I've mentioned, our ability to observe human responses given the fact that we can't give out large numbers, large enough numbers, is very bad down here. I don't know how this is going to be resolved, but on the other hand, in terms of environmental exposures to hair dryers and schoolhouses and so forth, this is what we are interested in. So the very thing we like to know about, we can't,

A. (cont'd.) from observation. So I think that is what I said.

Now, you asked a question, what point do you start to get an inflection in a lifeline. Now, I can take this little thing, I can say okay, let's re-expand this graph and see what... what if we took this and magnified it, what would that look like, and that would be the very curve you are describing if you were to magnify these things. You would get this sort of a curve, which is one of those I showed, and this thing might be...I don't know...where we start getting on the flat place that may be something...I said less and you want to say point five...you might, this might be what this thing looks like, and we simply don't have any experience down here to decide whether that's true or not.

But if you look at one of my papers in which I plot the time to tumor for a variety of dose levels, you can get some idea of where you start to get beyond man's lifespan, in that particular paper.

Now, I should add that the model, that model I developed was just a conceptual model and that the numbers that I put in the model were simply numbers to illustrate how it would work. Like, the girl I worked with, named Vivian Henderson, and I talked a lot about this, and most of our conversation has to do with sort of tuning the model. In other words, getting the numbers right. I think the concept is right, but we never were sure what the numbers ought to be.

One of the reasons that I never publicized or published that extensively was, we never could quite adjust the, get the numbers where we were happy with them. I even withdrew one in galley from a journal once. I think it shows up in my CV as an in-press. I forgot to take that out, but in fact it was accepted in galley...the only time I ever sent a galley back and says, throw the whole thing away. I just felt

A. (cont'd.) that I just wasn't sure enough about it.

Now, the South African publication, if you get that thing, is the latest version of the model, and this is in that publication, this whole business.

But I think that's a point. I think that we don't know what does go on down there and my feeling is that maybe we ought to play it safe. You know, I've felt, let's just go ahead and draw the line like that, and recognize that there's differences between what might be true and what we are estimating to be true. But if we are talking about protecting the public, maybe we are putting in a little margin of safety here.

The numbers of cases caused here on the leaner model, I showed you with a slide, is so tiny anyhow. It's not going to affect, you know, the way we spend our money or maybe our activities very much. I don't think that we are talking about creating very many cases. That is, a difference between these two things is not going to be terribly great in this very, very low area. Even now in the linear model of five nanograms in the general environment we are producing...whatever I showed, deaths a year, in the whole two hundred and twenty-five million people.

I think that's another reason that I haven't pushed this, because it turns out that the model takes effect in a range where it doesn't make very much difference anyhow in terms of the extra cases that are produced by the linear model in that range.

Q. Let's take a look at just what you are talking about. In exhibit two, in table three of exhibit two, that's where you have set forth all of your calculations. This is your Coldsprings Harbour paper.

A. Okay, yes.

Q. In that paper you set forth all of the components which get you up to the four thousand cases?

A. Yes.

Q. Now, when you made those calculations, you did them on a linear basis, not making the calculations on the assumption that the dose-response curve might actually have an inflection at a half, or one or something like that, correct?

A. Right.

Q. If we take a look at that and we talk about the low-exposure people in that group...let's say auto mechanics and other exposed workers, your construction workers...it's those people where this effective...a different actual dose-response curve would appear? Is that right?

A. Right.

Q. So that with respect to those people, it would be fair to say your linear assumption is that a conservative assumption there, you may well have a dose-response relationship which produces less disease and less death than you calculate? I'm referring to the very last...

A. Yeah, perhaps I...that's right. I think there is a slight, there might there be a slight overestimate if this is true, but remember, they didn't produce very much cancer anyhow. They don't make very much difference.

Q. Right. And what we are talking about here, two hundred and twelve cases for the automobile mechanics, and four hundred and thirty-eight for the other exposed workers?

A. Right. And if I cut those in half, I am not going to be so far off anyhow. I still say about one percent of all cancers due to asbestos, even if you cut those two groups down a little bit.

Q. If you cut those two groups in half, I guess we've got to remember that you multiply that number by fifty percent in the other...if you cut it in half you are talking about

Q. (cont'd.) three hundred cases, and then if you...that three hundred in turn is multiplied by fifty percent when you have your other asbestos cases, so we may be talking about another hundred and fifty. We may be talking about, say five hundred total?

A. Yeah. It might reduce the total from four thousand to thirty-five hundred. Yes.

Q. Let's talk about another thing that you've mentioned - what you call the multiplier effect of asbestos. Is that what the cancer specialists talk about when they talk about promoters?

A. Right.

Q. We are talking about one and the same thing then?

A. Well, I guess what they are talking about. This is all a little fuzzy sometimes, what they mean by promoters. I don't know whether asbestos is promoting...let's take the cigarette thing, to make it easy. I don't know if asbestos promotes the cigarette effects, or if the cigarette effects promote the asbestos effects. I don't know which way this thing is really working.

If you do some studies which you think will discriminate between initiators and promoters, it seems like asbestos always sort of comes out on the fence. It doesn't look like either. You can't tell what it is.

But you are right, that in the usual context, talking about a promoter, it would work like this. It would perhaps multiply something in this way.

A promoter...if it were a promoter, a true promoter, you should see a fairly rapid decline in risk after the exposure stops. The trouble is, asbestos doesn't fit this very well because if it resides in the lung for long periods, you don't know what you mean by stopping exposure. There isn't...

A. (cont'd.) that concept doesn't work as it would be in the chemical industry, for example, so you can't test it by the usual methods.

Q. But perhaps it is a promoter?

A. It is a promoter if that's what we are talking about here.

Q. Is it possible that we don't know what asbestos standing alone actually does do? That is, it may promote disease in the general population, as you've talked about for urban population, or it may promote cigarette smoking - lung cancer, but in terms of saying what asbestos and asbestos alone does, do we have definitive answers on that question?

A. No, I don't think we do, and it leads up to the fact that we don't know exactly why it causes cancer. However, there are some experiments going on that should be about ready to be published, that do contain an answer to this, I think. You will probably hear about these.

One of the ways to answer this would be to test the theory that it is simply the physical property of asbestos that produces a tumour.

Now one way you could test this would be to produce a substance that has the same physical properties as asbestos, but which isn't. For example, fiberglass. You could make manmade fibers to the dimensions of asbestos, and with animal experiments see if you can get the same effect. In fact, there is an experiment like that taking place. It was delayed for a long time because of the technical problems in producing something with the dimensions of an asbestos fiber.

Now at that...so, I'm kind of looking forward to that. If it turns out that these manmade fibers have the same effect, then we'll have to say that asbestos does this all because of physical properties. It will weaken the promoter thing, perhaps.

A. (cont'd.) If it turns out that these manmade fibers do not have the effect, then we have to look someplace else. Since asbestos has the property of selectively absorbing various materials, and is an excellent filter for this reason, you know, one argument has been that perhaps the asbestos simply absorbs the goodies off the cigarette smoke and somehow magnifies their effects on the lung.

So downstream someplace, we should know the answer to this.

Q. Regardless of whether we now know or maybe ever will know precisely what the mechanism is, we do know that this multiplicative effect is taking place?

A. I think that's safe to say that it is, right.

Q. Now, if you are looking at the absolute numbers...not in relative terms, but absolute numbers, is it true to say that the majority, perhaps even the great majority, of lung cancer cases among asbestos workers are in workers who smoke?

A. Oh, absolutely, right.

Q. Do you have any idea from your experience what that proportion of lung cancer cases would be...let's say, first of all, in your cohort who were smokers?

A. Well, it would be the same as the proportion of the general population...I think. Let me think about that. It would be very, very high. I would have to do some calculations. I back off from that.

I think it would be...it's rare to see a case of lung cancer in my asbestos workers who is not a smoker, which is one of the reasons it took so long to find out about this multiplicative effect, this sort of thing, delayed...it all comes from, mainly comes from Irving Selikoff's data, and for many years he didn't have any deaths among his nonsmokers, which led to a feeling that perhaps if you didn't smoke, asbestos wouldn't

A. (cont'd.) cause anything.

Then as time went on, since he had a large population, he did start to produce some cancers in the nonsmokers, and when the numbers got big enough, then I think this multiplicative thing shows up in the data that he has.

Q. But it's a...

A. It's certainly rare. Well, in the general population you rarely see a lung cancer case in a nonsmoker.

Q. So the proportions should be about the same? That is, if...

A. I would have to think about that. I think it probably...I would have to do a little calculation on that. Let's...for the moment, let's say it's about the same or perhaps even greater. You're talking about absolutes now, we have been talking about relatives, so you've got to change gears a little bit, and think about it.

Q. That's why I underscored absolutes when I said it, so that we would talk about real people rather than numbers.

A. You don't have a blackboard in here. What kind of a school is this anyhow?

You can't write on little pieces of paper, but there are numbers that go with this notion, and I think if you see the numbers, the notion might be better...see what the real numbers are like in this whole thing.

Next time, get a blackboard.

Q. Feel free to start scribbling up there, if you want. Don't let me hold you back.

MR. LASKIN: Maybe you could do it over the lunchhour.

THE WITNESS: Maybe that would be good.

Where are we?

DR. DUPRE: Well, would this be an appropriate

DR. DUPRE: (cont'd.) time to break?

THE WITNESS: This is just fine.

DR. DUPRE: Just before we go, Dr. Enterline, your plans call for you to be at the airport in the early part of the evening, so that we are talking about what...about five o'clock you would have to leave?

THE WITNESS: I don't know. What time is my plane?

MR. LASKIN: Six-fifty.

THE WITNESS: Six-fifty.

MR. LASKIN: Five, five-thirty, probably.

How long are you going to be, Ed?

MR. WARREN: I would say not a whole lot more. I'm not going to drag it out too long.

DR. DUPRE: What I'm really seeking to ascertain, counsel, is whether all parties feel comfortable, and Dr. Enterline, to our resuming at two-thirty, on the clear understanding that we would not go beyond, say five or five-thirty.

If you don't have that feeling, maybe we should be reconvening, you see, around two-fifteen.

MR. WARREN: I think for this counsel it's easy, because I've had a good part of my shot.

MR. LASKIN: I think if we add up everybody in this row back here, we come to about an hour and a half, but lawyers are not always the best people in the world to take time estimates from.

DR. DUPRE: Let me compare with my colleague from Kingston here.

MR. LASKIN: May I suggest a compromise of two-fifteen?

DR. DUPRE: Is that agreeable?

(REPORTER'S NOTE: General comments of agreement.)

THE WITNESS: Counsel, if you want to pursue this, why don't you get the retributability paper which has the tables in that I was going to...

MR. WARREN: I'll take a look at it at lunch time.

THE WITNESS: Yeah, and maybe if anybody is interested, they can see how it works out.

DR. DUPRE: We'll resume at two-fifteen.

THE INQUIRY RECESSED

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THE INQUIRY RESUMED

DR. DUPRE: Mr. Warren, would you proceed, please?

MR. WARREN: Yes, sir. I have a terrible tendency to refer to you as Your Honour, just because...

DR. DUPRE: Don't you dare.

MR. WARREN: If I do, please don't be offended. It's just that I can't look in the direction of a bench without saying Your Honour.

DR. DUPRE: That's an occupational liability.

MR. WARREN: I'm afraid it is.

DR. DUPRE: It would certainly be a liability here, let me assure you. So you watch that.

MR. WARREN: So when I slip into it, because I'm sure I will, please recognize why I do it.

PHILIP ENTERLINE, PREVIOUSLY SWORN, RESUMES THE STAND

CROSS-EXAMINATION BY MR. WARREN, CONTINUED

Q. Dr. Enterline, did you have a chance to take a look at your retributability paper at all during lunch?

A. I remember it, yes.

Q. Why don't we turn to that paper and I would like to ask you a few questions based on that paper.

For everybody's benefit, that is number eleven in

Q. (cont'd.) exhibit number one that Mr. Laskin put into evidence.

Dr. Enterline, let's take a look at table two in that study, because I first of all want to make sure we understand what table two is really saying.

First of all, table two is based upon...am I right... first, Dr. Selikoff's insulation workers, and second, data on rates of lung cancer from the American Cancer Society?

A. Right.

Q. From Dr. Selikoff's study, we see that there are annually three hundred and sixty-two cases of lung cancer mortality for every one hundred thousand workers in Dr. Selikoff's study? Correct?

A. Right, yes.

Q. For nonsmoking asbestos workers, the rate is forty point four workers per hundred thousand, is that right?

A. Right.

Q. Now, let me see if I understand these tables correctly. If one compares smoking asbestos workers with smoking members of the U.S. population, asbestos enhances that rate by a factor of four point nine for the exposures in Dr. Selikoff's study?

A. Right. That's right.

Q. For nonsmokers the multiplier, I believe, as you called it, is closely comparable - it's four point four?

A. Yes.

Q. If we had a hypothetical cohort of a hundred thousand people, which we obviously are never going to have one that big, but it means every year we are going to be losing three hundred and sixty-two workers on this hypothetical-calculated basis..actually these are his rates...so we would be losing...if they were exposed to the asbestos fibers that Selikoff's workers were exposed to, we would have three hundred

Q. (cont'd.) and sixty-two smokers who would die of lung cancer?

A. Right.

Q. And about forty nonsmokers?

A. Right.

Q. So we have got about ten times as many smokers... about ten times as many of our lung cancer cases are among smokers as nonsmokers?

A. That's right.

Q. Now, my first set of questions is to try to figure out where we are today. In other words, kind of recalculate Dr. Selikoff's table using your relative risk factors. Your relative...I believe you calculate in one of your exhibits this morning, or one of the exhibits I have previously seen, a relative risk of about one point two at two fibers?

A. That's right, yes.

Q. So if we use one point two as our relative risk for the whole population of workers in my hypothetical cohort, and apply that relative risk back into the number of workers hypothetically dying, we can figure out how much they would be, correct?

I'm making it a little too complicated, I think.

A. What are you trying...

Q. I guess what I'm trying...

A. Let's go back over it now. I've got an estimate of a relative risk of one point two, I think it's an SMR of one twenty-one, something like that.

Q. Yes.

A. For two fibers for fifty years.

Q. Right.

A. Right. Okay.

Q. Now, let's assume...

A. Based on...that happens to be based on my own study

A. (cont'd.) of retired workers.

Q. Right, I understand that.

Let's assume for a minute something that Dr. Selikoff's work suggests, and that is that the multiplier is roughly comparable for smokers and nonsmokers?

A. Right, okay.

Q. So let's assume that your one point two as a multiplier applies to that portion of your population who are smokers, and that portion who are not?

A. Right, okay.

Q. Now, let's assume again for a minute that Dr. Enterline's numbers are right...

A. Selikoff's numbers are right.

Q. But let's assume...I am going to try to apply your relative risk numbers back into Selikoff's population. In other words, the assumption being that exposures have been reduced down to a level which produces the relative risks which you observe...namely one point two.

A. So these marginals, instead of four point nine would be one point two?

Q. Yes.

A. Okay, okay.

Q. So if we did that and if our relative risks now became one point two...

A. Right.

Q. ...we could simply divide the difference between four point nine over one point two, into three hundred and sixty-two, and come up with a rate of asbestos insulators who would die of lung cancer, based on a linearity assumption?

A. Well, just multiply the U.S. rates by one point two, and you'll have the insulator rates.

Q. Same thing?

A. Yes. But easier.

A. (cont'd.) Okay, so...all right.

Q. In other words we get...

A. So you get eighty-six and eleven, or something like that.

Q. We get approximately eighty-six there...it's the same thing, right. And we get approximately eighty-six there and we get approximately, say, eleven for nonsmokers, right?

A. Maybe. Something like that, yeah.

Q. So the numbers that we have seen...I guess my first point is, we have seen from the time of Dr. Selikoff's study a substantial reduction in the number of anticipated annual lung cancer cases due to the reduction of exposure down to levels of around two?

A. Well, I don't know. We haven't seen it. We've talked about it.

Q. Yeah, we would expect...

A. We don't know...we don't know what the risk is at one point two. We think we know, but we...

Q. Right.

A. These are observational data. My calculation is purely theoretical.

Q. I understand that, yes.

Again, I recognize, and we all have to recognize, that extrapolation is a dangerous business and that none of these extrapolations are more than extrapolations, but if we assume for purposes of this exercise that one point two is a good number and that's the number you have calculated, we would be estimating lung cancer deaths at a rate of eighty-six, I think you say?

A. I didn't...you mean anybody with a little calculator multiplying seventy-four by one point two, and see what you get.

UNIDENTIFIED SPEAKER: Eighty-nine point nine.

MR. WARREN: Q. Eighty-nine point nine, my calculators tell me, and I guess eleven is about right, too.

THE WITNESS: A. I just guess that, but I knew it was somewhere around there.

Q. If we take eighty-nine point nine and eleven, we are saying about a hundred people here who would be contracting lung cancer as a result of two fiber exposure?

A. Mmm-hmm.

Q. Now, first of all, of that one hundred...

A. Of the eighty-nine, you mean?

Q. Yeah, and there would be eighty-nine and then there would be eighty-nine workers in my hypothetical cohort who would have contracted lung cancer experiencing exposure of two fibers?

A. Yeah.

Q. And approximately eleven or so, ten or so, of the nonsmokers, right?

A. Yep.

Q. If you add those together, you have about a hundred cases?

A. Yep. Well, your adding them together assumes that there are exactly the same number of people who smoke as who don't smoke.

Q. That's right.

A. Okay, if you want to assume that, then you can add them together.

Q. That assumption...I was going to relax that assumption in a minute, but making that assumption we've got about a hundred.

Now, keeping that assumption for a minute, of that hundred people, a number of...

A. Wait a minute, wait a minute. No, when you add them together...first of all, these are...each one of these

A. (cont'd.) is a rate per hundred thousand.

Q. Right.

A. If you add two together, then you have a rate for two hundred thousand. The denominator...let me see now...yeah, because you are doubling the denominator, you see, when you add them together. So you've got a hundred thousand people that smoke, and you get eighty-nine, right? That's what you said?

Q. Yes.

A. Now, you have to have a hundred thousand people that don't smoke, the same number, you get eleven, okay? So it means all together then you've got two hundred thousand people and a hundred cases.

Q. Right.

A. A hundred thousand smokers, a hundred thousand nonsmokers, and a hundred cases. So you've got...the rate...if you want to know what the overall rate is, it's fifty per hundred thousand.

Q. Right.

A. Okay?

Q. Now...exactly. Now, you still have...I'm going to try to keep with absolute cases so that we can sort of understand where we are...you have a hundred absolute cases, as you say, you've got two hundred thousand...

A. People producing them.

Q. ...people, because your cohort is a hundred thousand of smokers and nonsmokers.

A. Right.

Q. Now, of that hundred people, a large number of those people, the majority of them, would be dying of lung cancer anyway simply because the background population incidence is what, seventy-four plus nine?

A. Yeah, well...exactly eighty of them would have died anyhow.

Q. Yes. So twenty of them...

A. Is that right? It's almost right, yes.

Q. Yeah, but again, talking about the number so we can follow each other...

A. You kill off twenty with asbestos and eighty would have died anyhow, so you've got a hundred deaths, roughly. It isn't quite right, but it's close.

Q. Now, I would like to go through two different sets of exercises here. I would like to see what the relative utility of two different approaches to this problem might be.

First of all, let's assume that instead of reducing asbestos fiber level exposures to two fibers, you reduce them to point two fibers. Now, if I understand the mathematics correctly, that reduces the relative risk if you make the linearity assumption to one point zero two.

A. Okay, yeah.

Q. All right. Now, if you apply the one point zero two and calculate the number of lung cancer cases we are going to have in our population here, you are going to have one point zero two times seventy-four point four, and one point zero two times nine point two.

A. Mmm-hmm.

Q. Correct?

A. That's right. Did you multiply that out? You get a few extra cases there, I don't know, not very many.

Q. Yeah, you get a few extra cases is what it boils down to.

A. It's...what is it...two percent. In other words, you've got a two percent excess in lung cancer at point two fibers.

Point two fibers produces a two percent excess in lung cancer, is what we are saying.

Q. That's right. So, the one thing I didn't do

Q. (cont'd.) is bring my calculator, so I'm kind of at a loss here to actually do the calculations, but...

A. Well, so you get seventy-five and ten, or something like...

Q. Yeah, seventy-five and ten..if we can recapitulate for a minute, we are probably talking about...out of our two hundred thousand workers, if we have a one point two relative risk, we probably have a hundred cases, of which about seventeen...that is a hundred minus eighty-three, eighty-three being sum of seventy-four and nine...would be excess cases as a result of asbestos exposure, and if we do the same thing with point zero...one point zero two, we've got a few additional cases that we've saved, maybe tennish, something like that.

A. mmm- hmmm.

Q. Now, what I would like to ask is, what if we could reduce smoking in our cohort, not entirely - just by a third. Let's assume we can reduce to make this calculation simple, let's assume that one-third of our smokers become nonsmokers. What happens to the absolute number of cases that we saved?

A. Well, it's easy enough to calculate in the same way we did for the asbestos.

Q. Right.

A. You have to have a little calculator if you want to do that.

I mean, if your point is which is more important, smoking or asbestos, quite obviously the relative risk of smoking is ten, and the relative risk for asbestos is five. Smoking is extremely important.

If you wanted to move the lung cancer rate, the way to move it is to change smoking habits, if you 've got to pick something. Now, can you do it, that's the question. You don't really have any leverage on that one, but that's

A. (cont'd.) obviously what you'd like to do.

Q. Do you have no leverage? I mean, if you make an assumption of linearity here, which all of these calculations are based upon, if the total cigarette consumption of your population of asbestos workers reduced by one-third, I take it we would be reducing the lung cancer risk very substantially because we would be dropping a third of the workers down from a rate of seventy-four point four down to a rate of nine point two?

A. Well, that's if you eliminate smoking. I mean, you are talking...I mean, that's U.S....

DR. UFFEN: The relative risk is four point nine to four point four.

MR. WARREN: No, those relative risks of smoking asbestos workers as compared to smokers in the general population, and nonsmoking asbestos workers compared to nonsmokers in the general population. What we are talking about is the difference between the rates of lung cancer for smokers and nonsmokers in the general population, which is in the middle column here. If you could reduce the number of smokers by one-third, what you would be doing, Dr. Enterline, am I right, is to be taking a third of the workers out of that smoking category where they have a rate per hundred thousand of seventy-four point four, and putting them down in a much lower rate category where they have a rate of nine point two.

THE WITNESS: A. You are talking about the U.S. males column. Do you mean to do that, or do you want to talk about the asbestos insulators? I'm not sure.

MR. WARREN: Q. Yes. I think I want to talk... I think either way you can do it. So let's do it your way, though. Let's talk about taking them out of that three hundred and sixty-two rate and putting them down into the forty rate.

A. Yeah. I mean, if your point is...I think your

A. (cont'd.) point is that the powerful variable here is the smoking, there is no doubt about that. If you could move that...and I haven't calculated this...but a given amount of movement there is probably going to have twice the effect as the same amount of movement in the asbestos category, because you've got a long way to go...you've got a relative risk of ten to play with in the smoking, and only five to play with in the asbestos.

Q. Okay, I think we are saying the same thing, because when we talked about this morning, if we take your cohort and we compare it with the two fiber standard, we had roughly at least...we've had a reduction of a factor of fifteen... from thirty to two, or the way I calculated it came out about the same. So we have moved a long way down with asbestos exposures.

A. From the exposures these insulators had.

Remember though, we did discuss the fact that the experience of these insulators is not entirely consistent with the experience of asbestos products workers, and I cranked that into some of my estimates and we discussed the fact that I don't know why the insulators have such a response.

One thing we didn't talk about was fiber type, which I expect somebody would want to talk about, but...in other words, using my asbestos products workers data to project into an insulator data set, there is one other problem, and that is that the insulator data set is not quite consistent with the asbestos products workers data set. There is a difference there.

But I think, you know, if the point you are making is what are the advantages to be gained by cutting down on smoking, they are obviously tremendous. Not only in terms of lung cancer, but in terms of heart disease and lots of other kind of cancer that seem to be related to smoking.

Q. I guess one more thing about the calculations

Q. (cont'd.) we have just been engaging in and... I wish we did have a blackboard because maybe we could make it clearer than maybe we've made it.

Throughout these calculations we have been assuming linearity?

A. Mmm-hmm.

Q. As we talked about this morning, when one...we don't know where the break point is, but there are reasons, not from proof, but reasons to believe that there might be an inflection in the dose-response curve at lower levels. So that the effects of reducing asbestos exposure below the present levels are likely to be very conservative.

That is, they may well be an overestimate of what the actual effect...

A. The effects may be less than we project.

Q. Than what we calculated.

A. Why don't we...let...You know, just to clarify this, let me put something up here that might help a little bit.

The trouble is, you have a funny number system, and it's hard work. Make a simple number system out of it and let's suppose that we have smokers and nonsmokers...we'll just have S4 as a nonsmoker...and we have asbestos workers and nonasbestos workers...we'll make it A4 for nonasbestos workers.

Let's put down some rates that have the right relationship to each other, but it's an easier number system.

Q. Sure.

A. Okay? Let's suppose that we have a hundred here, and so of the asbestos workers we say that...who smoke, we have a rate of a hundred, and let's say that of the asbestos workers who don't smoke, we have a rate of ten. And let's suppose of the people who are not exposed to smoke, we have a rate of twenty, and of those who are not exposed who don't smoke, we have a rate of two.

A. (cont'd.) Let's just calculate the relative risks involved in this thing. Now, it's clear that if you move across the board that the difference, the relative risk imposed by smoking is ten for the asbestos workers...we'll call this relative risk...and ten for the nonasbestos workers, who are not exposed.

The risk imposed by the asbestos is five, and the risk imposed by the combination is the risk...moving this way across the table, which is fifty, okay...it's really five times ten.

I think this is about what the table shows, except his numbers are different. But, I mean, this is to clarify. I think this seemed to be the result that Selikoff got.

Now, let me say something about this. First of all, Selikoff didn't observe any deaths here for a very long time. This ten...let's suppose this was the real number...didn't show up. Remember I said earlier that for a long time people thought that if you were an asbestos worker and you didn't smoke, you didn't have any excess. The relative risk was no risk. That, in fact, led to the belief that maybe a guy who worked for an asbestos company and doesn't smoke and gets cancer couldn't have got it because of the asbestos. A lot of those cases, I think, were not awarded claims when they came up.

Now, why didn't he observe these for such a long time? I can cite it in the study of uranium miners. A few years ago we observed almost exactly this in uranium mining - the multiplicative effect of radiation interacting with smoking.

The uranium miners study, if you go back away, there are kind of more mature studies than in the asbestos workers study. In the world today there is kind of a joke that half the papers are about uranium, and the other half...if you add in the asbestos papers, you get ninety percent of all the papers

A. (cont'd.) ever written in occupational epidemiology, and we are catching up to asbestos. In other words, we are going to catch up to the guys in the uranium business one of these days.

But, a lot of the literature on occupational health is just these two fields.

But we are running a little bit behind uranium miners, but an interesting thing has happened. It turns out in the uranium miners, this relationship which existed perhaps ten years ago and was widely publicized, is now disappearing. Why is it disappearing? It appears that the nonsmoking uranium miners, this group, is now beginning to experience a very high incidence of cancer.

Now, why would that be? Well, maybe the Druckley theory is really right. Maybe it's a waiting game and maybe the interaction between, in that case smoking and radon, and nonsmoking and radon, is like a dose-rate effect, so that what we are seeing in the uranium miners is the disease incidence further downstream.

So this table has just been kind of wrecked in the uranium miner thing in the last ten years, because we are now beginning to see cancers in nonsmokers that we never saw before.

In other words, uranium is producing the cancer, okay. But if you combine the radon faction with the smoking, you've got the cancer earlier than if you left the radon work all by itself without the smoking.

So, while I did publish this and this is absolutely so in 1981, I don't know what it is going to be like in 1991. You know, I think that...as I say, there are little bits of evidence to suggest that maybe Druckley is right, and two bits I've just given you.

A. (cont'd.) One is that Selikoff never observed these cases until just very recently and kept publishing this stuff that made people think that maybe smoking is the factor, and if you don't smoke that asbestos can't hurt you. The problem is, the experience we have had with another substance even longer studying it and we are further downstream in our studies, suggests that this might not hold forever. It might start changing. I think time will tell, we'll find out the changes. If we start now to see a much greater excess, what should happen if that's true is, as time goes on this relative risk will start to get big.

No, this relative risk will start to decline. In other words, we are going to start to see an unusual increment right here - this time to tumor thing. This is going to start getting to be a big number up here.

DR. DUPRE: I just want to make sure I've followed this, Dr. Enterline. You are suggesting, as I understand it, that it is possible that we may witness a downstream effect among asbestos nonsmokers...

THE WITNESS: A. Right.

DR. DUPRE: As we apparently have already witnessed among nonsmoking uranium miners?

THE WITNESS: A. Uranium miners, right.

DR. DUPRE: Thank you.

MR. WARREN: Q. First of all, the evidence today is as reported by you and Dr. Selikoff, right?

THE WITNESS: A. Right.

Q. And as I see it, this table you put on the blackboard is basically republishing in a simplified, rounded-off form essentially what is in table two here?

A. Right, yes. Just numbers you can see better.

Q. Yes. Now, what has happened in the real world with asbestos is that because we have reduced exposures to asbestos

Q. (cont'd.) so substantially over the last twenty years, those relative risks of five there have dropped away down? That is, the relative risks of asbestos and nonasbestos workers, whether smokers or nonsmokers? That much is true?

A. Well, I don't know. I assume it's true.

Q. Yes, you assume. Your calculations would indicate that it's true?

A. Yes.

Q. And your calculations, again derived not from Dr. Selikoff's cohort, but your cohort, bring you down in the neighbourhood of a one point two relative risk for two fibers?

A. No. The one point two is just based on a regression equation.

Q. Yes, I understand that.

A. Yes, okay.

Q. But that...I understand that and I realize we are all handicapped here by having to deal with not real life, but some mathematical artifact of real life, namely extrapolations, and when I say things as if they were true, I don't mean to say that they are true, but rather that that's what the calculations show.

With that assumption in mind, the one point two is the risk that you've calculated?

A. Right. If you could really compare products workers with insulators, and if insulators had had an exposure of two fibers, then those numbers would be one point two instead of five across the bottom.

Q. Right.

A. That's my guess.

Q. Now, I'm going to ask only two more questions, I think, to summarize this whole thing.

First of all, the calculations you've made, Dr. Selikoff's made essentially, and certainly these, are based

Q. (cont'd.) on that linearity assumption?

A. No, these aren't. This really has nothing to do...

Q. This is observation?

A. This is something else.

Q. But if you take that one point two relative risk and you calculate the number of cases, we would be calculating cases based on linearity assumption?

A. That's right, yeah.

Q. Any further reductions in exposure, and therefore any further calculated reductions in cases, are based on linearity assumption in our discussions so far, right?

A. By and large, yes.

Q. Right. But if it turns out that the linearity assumption is in some real world that we can't see, but certainly somehow exists, if it turns out there is an inflection in the dose-response curve and we are tailing off that disease at the low levels in a disproportionate way, it may well be that we can reduce all or nearly all of that disease by only marginal additional reductions in the exposure?

A. That's right. The increments...if that were true, that is if it's curvilinear at the low doses, the increments in reduction would be greatest with the first increments in dose reduction. In other words, the reduction increments in disease will be the greatest in the first increments in exposure. Later on there is going to be not much payoff in disease, if that is true, if it's curvilinear.

Q. All right. And we discussed this morning...I've got only about three more questions, I'm overstaying my welcome a little bit here, and I'm going to stop...

A. You had two a minute ago. You're getting longer.

Q. I know, but the one became two or three, and now the one or two remaining have become two.

DR. DUPRE: These are rabbit questions. They multiply.

MR. WARREN: That's right. It's kind of like the multiplier effect. Asbestos has that effect on lawyers, too, I guess.

But at any rate, the two observations we made this morning about nonlinearity at low doses were...it could be, it is consistent with your data on intermittentexposure, first of all, and it is second of all consistent with the Druckery hypothesis and as applied in the human setting?

THE WITNESS: A. You mean the nonlinearity...

Q. Yes, the nonlinearity at low doses.

A. Well, it's consistent with the Druckery hypothesis. But what was the other one?

Q. The other one was, it is consistent with your observations of high, relatively higher disease for intermittent exposures?

A. I don't see how. I mean, I don't see that's involved. I mean...

Q. I thought when we went through this morning, I thought that if the high exposures are overwhelming defence mechanisms, then...and what high means is undefined... that when defence mechanisms are in operation there can be a relatively greater protective effect.

A. Okay, all right. I guess so. I guess if you had continuous exposures of two hundred fibers, you might get some extra effects. In other words, the high end, true, it might be nonlinear if you get up to that overwhelming effect area. True.

Q. One other thing we haven't talked about this morning, but you alluded to was...

A. Let's put that down. That's an interesting thought.

Q. Yes.

A. So that what you've really got, if you think about that, is a dose-response curve that is down in some little areas, kind of flat in here, you can't observe that, and then it starts up and it's fairly straight like that. And then you get this part...then it starts climbing very rapidly here. You suddenly get another inflection point here where the defence mechanism is overwhelmed, and here where you...the effects start to occur beyond your lifetime. So you get two inflections in that.

Q. Mmm-hmm.

Okay, now, the...one thing we didn't get a chance to get a chance to talk about this morning, but which you did comment on in your...

DR. UFFEN: Could I interrupt?

MR. WARREN: Sure, because I'm going to a different subject.

DR. UFFEN: The reason...in the earlier slides, and slides we have seen previously on dose-response curves, it always seemed to me that the actual observation had a plateau, so that if there is a second elbow it isn't for a greater rate, it's for a lower rate? Am I right? That that's what the evidence has been.

MR. WARREN: Q. Yeah, that's what I was a little confused at, as a matter of fact, too. Why shouldn't it be an S, why shouldn't it go like that?

Is that what you are saying?

DR. UFFEN: What I'm saying is, that it might go either way, but the evidence that has been put in front of us from epidemiologists is that it levels off, which means that the greater the dose, the less response. You've overwhelmed the system and you can keep on...it's an overkill.

MR. WARREN: That's what I just thought, too.

THE WITNESS: A. Well, what we just said...first of all, we are trying to understand why the maintenance worker who has the same average level of exposure, say, has twice as much cancer. One theory might be that since this exposure comes in bursts, it has an extra kind of effect and overwhelms the system and you get extra cancers out of that.

I think that would mean that when you get up to this level...it might be, say, two hundred fibers, you begin to get kind of extra effects. Where you are going along here, apparently, you are somehow being able to...the clearance mechanism seems to be working. You suddenly overwhelm it, so you get this happening.

I don't think this has anything to do with reality, you know. We are just talking theoretical kind of stuff. We are just pursuing what you might get.

I agree that the data don't show this at all. But I think perhaps for a different reason. I think that...

MR. WARREN: Q. One of the difficulties, I think... and maybe you can correct me if I'm wrong on this...one of the difficulties in trying to graph this is that we are always graphing cummulative exposure, and what we are hypothesizing is bursts of exposure, which are kind of hard to get on our graph since our X axis is cummulative exposure.

THE WITNESS: A. Yes. Well, of course, if you express the bursts in, as they must have had to do with the insulators, as an average...and once again you can get Bill Nicholson and you might figure out he did that...this is kind of a trick...that equation to put it on his scales is divided by the times times average...

Q. Sure.

A. Now, I...you know...all the stuff we have, you see, is like in this area. I've presented some graphs that are kind of pieces of a picture, and we are trying to decide

A. (cont'd.) what is at the other end that we haven't had a chance to look at.

I don't think...

Q. One final topic that I've been trying to get to. One thing we haven't talked much about is asbestosis, but is it fair to say that as a practical matter, at current much lower exposure levels, that asbestosis is much less a matter of concern than it has been historically?

A. I think clinical disabling asbestosis, probably yes. As I said earlier, if you accept the very slight radiographic changes, you do seem to see, though, at low exposure levels, but the significance is not clear. I don't know what happens, for example, if you looked at tissue at autopsies.

You can define asbestosis in lots of ways. Maybe we all have a little bit, a little fibrotic change...who is to say. If you sliced us fine enough, maybe somebody would locate that.

I think, you know, I think the thrust of your question is, do deaths from asbestosis seem to be much of a problem. Well, not, you know, it's not a big part of the deaths that you see in these studies that have been done, and you know there is a theory that some people still hold to, and that is that you cannot develop an asbestos cancer unless you have asbestosis. I think even today there are a few people who think that in the absence of asbestosis defined in, probably some microscopic way, you cannot get cancer from asbestos.

So, asbestosis is a little complicated sort of a problem to talk about, I think.

Q. But asbestosis mortality...

A. You can say the slide I lost shows the number of deaths every year is not very great, in the United States, from asbestosis.

MR. WARREN: I think that's it for me.

DR. DUPRE: Mr. Bazin, do you wish to proceed next?

CROSS-EXAMINATION BY MR. BAZIN

Q. Mr. Enterline, if we could for awhile try and bring back the picture to Canada, to find out...I understand you worked at McGill for a couple of years in 1965, 1967?

A. Yes.

Q. In the same type of work that you are presently involved in?

A. No.

Q. What were you doing...

A. No. Curiously, I didn't do any of this while I was at McGill. I was brought there as the professor of medical statistics. They had never had one before, and I developed a number of teaching programs for the medical faculty, for the staff, at the hospital and the medical school, and I got a grant to study lung cancer among Jews in Montreal, and wrote a paper on it with a guy named Isadore Horowitz, and very curiously I had almost nothing to do with asbestos when I was there.

I think it was because, you know, there was a very clearcut job to do. Corbett had brought Charles Rossiter from England to be a statistician. There was just simply nothing for me to do. It was all being done, and I had a lot of other things to work on.

Q. If not between 1965 and 1967 when you were at McGill, in the course of the work that you have done in the field, did you have a chance to examine, quote, unquote, the Canadian scene? In terms of either cohorts, in terms of statistics, in terms of what is being done with asbestos in Canada?

A. You mean in terms of the studies that MacDonald did?

Q. No, no. In terms of your own experience and your own findings?

A. I haven't tried...I haven't extrapolated any of my findings to Canada particularly. Part of my cohort, of course, were Canadians. I had a plant here in Toronto, and I have the mine and the mill at Asbestos, and the little factory at Asbestos.

Q. That's part of the cohort that was discussed this morning, is that what you...

A. Yeah. Part of...the original retiree study had all of the retirees from Johns-Manville, including its Canadian operations, and I in fact did a little..I did some tabulations on, for example, the factory at Asbestos versus the mine and mill at Asbestos, in one of my early reports.

What happened was, then I had no more contact with the Johns-Manville Corporation, and I updated it and I had to leave out the Canadian part because I had no way of tracing those people. So a later report of mine leaves the Canadian cohort out.

Q. Your later update leaves that Canadian section out?

A. Yeah. Yeah.

Q. Could you tell the Commission if it is fair to compare, at the times you did, the SMR U.S. with SMR Canada? Would we be talking aboutthe same type of numbers?

A. I don't think they would be very different. I don't think that introduces much of a bias.

I think that one of the factors in the Canadian report on miners is the fact that these tend to be kind of rural people, as compared to, say, people in Montreal or New York City, and I suspect that if I were to have done separately very much with the work around Asbestos...that is, the mines, the mills and the factory, I would have used some local death rates

A. (cont'd.) there to calculate an expected...

Q. You would have used local information if available?

A. Yeah.

Q. This ties in to what you mentioned this morning as being the ideal?

A. Right.

Q. Taking the local picture, if available?

A. Yeah.

Q. The regional picture. If some investigations had been carried out in the past five years showing a comparison between local populations and asbestos workers, would you agree that that information would be more conclusive than the information gathered so far?

A. You mean if MacDonald had used a local population for...to get his expected rates, rather than provincial population?

Q. Yes.

A. I think that would be better. I think I would have done it that way.

Q. MacDonald or another...

A. Anybody.

Q. Or anybody that would have been in a position to compare the local population with the asbestos workers.

A. Yeah.

Q. You would agree?

A. I would think that would be a better way to do it. I mean, I think that it's the population that produces the workers that should be looked at in terms of their expected...to get an expected disease rate.

Q. Have you ever been in the asbestos area of the Province of Quebec? You mentioned that you had looked at part of the overall cohort being J-M retired employees, but have

Q. (cont'd.) you physically been in that area?

A. Yeah, sure. Sure.

You see, before I went to Canada, I was with the Division of Occupational Health of the U.S. Government, which is the forerunner of NIOSH, and that goes away back to the 1960's. In the early sixties, I did visit both Thetford-Mines and Quebec, perhaps more than once. And at one time when I was in the government, part of what is now NIOSH, we were interested in doing just what MacDonald did, and I did a little bit of field work on that before I ever met MacDonald. That would be... that was going to be an update of a study by Braun and Trueon, which appeared in 1958, and which was really one of the first real epidemiologic studies. We, when I was in the government almost twenty years ago, so that's 1960...his study reported in 1958...we wanted to update that and so in connection with that I did visit the mines and met a lot of the people, and I think I was back after that.

But I know the areas, I think, pretty well.

Q. It would be, given your experience, is it reasonable to assume that there would be more exposure in a mine than in a garage, for instance?

A. I would assume so.

Q. Given the present or the past years' levels of exposures in various of these mines...and I refer here to the QMA brief, which has been deposited with this Commission... with asbestos, which would indicate levels below two fibers, between point five and two fibers, in an operating mine, how can you reconcile such numbers with a number that you used in your table three of the document deposited this morning, for auto mechanics and garage workers, of point three?

If there are in mines levels below point three, how can you arrive at the point three level...using that as an example...for your auto mechanics and garage workers discussed this morning?

A. I thought you said the levels were below two in the mines?

Q. Below...they would go between point three, point two, up to two.

A. Is that from mine to mine, or is that from area to area? I mean, where would you get point three?

Q. From a job station.

A. From a particular job?

Q. From stations within that mine, yes.

A. Well, it's conceivable that perhaps some parts of the mine may have no more exposure that the person that works in a garage.

Q. How could you...then how did you arrive at that point three specifically? I didn't go through the paper, but you did mention a kind of a ballpark figure for that?

A. No, that's a pretty good figure. I think I give references as to where I got that, but that has been pretty well studied. That is, they have done area sampling in garages. There are at least two papers on that. But I think I may have mentioned that probably is one of the better numbers in my paper, the work in the garages.

I think the best reference is the one I gave.

Q. All other exposed workers, again at point three?

A. Well, I said, that was getting kind of shaky when I got down to that part. That's just...that's my best guess. It includes...I think I mention people who were exposed less than a year, people who are carpenters, say, in a job where someone somewhere uses asbestos and they walk by once a day, construction workers. I suppose if there is any number that you would have to pick that would be the weakest, you've probably hit it. That's the tough one because that's the all other, which is a mixed...all I can say is that whatever you make that, it's not going to change the overall estimate an awful lot...any reasonable number you put on that is not going

A. (cont'd.) to affect my one percent a great deal.

Q. In the secondary asbestos products workers, your equivalent exposure level is set at four?

A. Yeah, that's a pretty good number, I think. I think that's from Weston Associates, and I believe that those are actual numbers that they got from the industry.

Q. In...again, using these numbers...is there any attempt made to find out what other cofactors may be,aside from smoking, which has been dealt with?

A. Other than the...well, now I'm not sure. I mean the numbers themselves,of course, has nothing to do with cofactors. Now, the effects of that, I, in this, didn't do anything. In other words, I didn't try to adjust for where these secondary products workers might be, you know, No, I didn't try to make an adjustment for that.

Q. Could I refer you to one of your articles titled, Asbestos in Asbestos Cement Workers? It's number two.

A. Yes.

Q. It deals with cement workers?

A. Mmm-hmm.

Q. Looking at the conclusions, page 183, there is a discussion about exposure to asbestos and silica for these cement workers?

A. Yeah.

Q. There is a conclusion that...and I quote..."It is probable that a level of total dust exposure can be attained which will be unlikely to produce the pneumoconiosis which had resulted from past higher levels of dust".

A. Yeah.

Q. You were then referring, of course, to the presence of both asbesdust and silica, is that correct?

A. Yes, asbestosis and so on.

Q. Yes. Is there...have you been able to ascertain, in the cases of lung cancer, what, if any, portion that can be made to the presence of silica versus the presence of asbestos?

A. I think it's pretty well established that silica exposure and silicosis are unrelated to lung cancer. That's a battle that was fought thirty years ago, and I think it's...I don't think anybody believes that silicosis is related to lung cancer.

Q. In silicosis alone?

A. Yeah.

Q. What about silica and asbestos?

A. I doubt if anybody would be able to find out what the combination does. I think the point here is, that if you do radiographs of people in the asbestos cement industry you get some very curious looking x-rays that reflect two kinds of dust exposures, and I think that's maybe part of the point of this, that these are funny-looking x-rays because you've got guys exposed to two things simultaneously. So you've got the nodulation you see in silicosis, and you get the ground-glass appearance you see in asbestosis, and I think it's an interesting phenomenon.

Now, I should point out this is a paper I wrote with Hans Wile, and of course we had to divide it up. When you get Hans, you might question him about some of that stuff. He didn't ask me about mine, I didn't ask him about his, we just stuck it together and wrote it.

UNIDENTIFIED SPEAKER: We'll tell him you claimed that was his section.

THE WITNESS: A. Yes. Anything wrong with the paper, talk to Hans about it.

MR. BAZIN: Q. On the question of cofactors,

Q. (cont'd.) be it in the garage as a workplace, or the cement industry, cement pipe industry, has there been, from your part, have you been able to ascertain the impact of cofactors on the health of people working...?

A. Well, now, we are excluding cigarette smoking?

Q. Excluding cigarette smoking, which has been dealth with?

A. Excluding background levels such as living in New York City versus living in Lincoln, Nebraska, this sort of thing? Well, I think there's some immunogenetic measures that have been linked to asbestosis, but I'm not a good person to talk about that...though I think I'm probably talking about predisposing. By cofactor, you mean interactive...other environmental exposures? I guess I don't know of...I can't think of any, you know, that would somehow exacerbate or moderate the effects of the asbestos.

Q. Coming back to the article which was discussed this morning, just to get the clear understanding, you are talking about a cohort or an alysis that goes up to 1970?

A. Right.

Q. You take a picture in 1981, and you draw numbers from that picture taken in 1981, of the 1970 population? Is that correct?

A. Right. I, in effect, stopped the exposure in 1970, let the people that would have died, die through the seventies, and then I say now, it's 1981, what do we have and what's the future likely to be for these people.

Q. And there is no examination as to what they may have done between 1970 and 1981 in terms of being exposed or not exposed...?

A. No, you know, I did...I think this is an assumption a few other writers made and perhaps that's the

A. (cont'd.) reason I did it was, they said, okay, now we licked the problem in 1970 by having OSHA, let's assume they really did the job and there's no more exposures. So let's stop the exposures, how many people are left in 1981, and then what will happen to those people in the next twenty, thirty years in terms of cancer and how does that relate to the total cancer problem in the United States.

Q. On this question of stopping it in 1970 on the assumption that the small window in the corner of your charts will be protected by the standards determined at the time, how does the question of latency become a factor in 1981 when you look at those figures? How do you bring that into your figure?

A. Well, it was built-in in the initial estimate of response. Remember the twenty-two percent figure and the fifteen percent figure were both related to excess twenty years or more after first exposure.

Now, again, I've...I guess I've assumed...let's see...I guess I assumed that in the first twenty years you wouldn't see much of a response, and so I must have built a lag in. I'll have to look at that. I did think about that, it's in there.

But the twenty-two percent and the fifteen percent are twenty years after first exposure, proportions, not diluted by the first day after exposure, a year, a month, and so forth.

Q. It's twenty years after exposure?

A. After first exposure.

Q. After first exposure?

A. Yeah. The lag period was built-in to the response estimates of twenty-two and fifteen percent.

As a matter of fact, I think all of Selikoff's data is reported in twenty years since first exposure. My

A. (cont'd.) retirees all had twenty...they averaged twenty-five years before they retired, so it was built-in to that. I think the Peto stuff, it's built-in to that, too. So it was handled in that way.

Well, okay, sometimes...sometimes you want it in, sometimes you don't want it in, in making estimates like this, but that's a little more complex and maybe it's not relevant.

Q. On the high intermittent exposure, was it... you made a comment this morning, I guess in reply to questions from Mr. Warren, about the high level of exposure for a short period of time, as compared to the low level of exposure for a longer period of time.

What would be...if we had a situation where you have twenty-five years at two fibers as compared to a situation where you have two years of twenty-five fibers, are you able, from your experiments, your work, to assess the risk in both situations?

A. Well, not in those kind of extremes, but one of my tables shows that the contribution of time is very close to the contribution of dose rate, and I concluded that... again this is in my retired population, remember, and this is a population where the dose stopped before I started the followup...and I concluded that the kind of relationship that you just mentioned, this is probably equivalent doses.

In other words, you said what's the difference between twenty-five years at two fibers or two years at twenty-five fibers. My conclusion was, they have the same effect, in a short answer, and I've got a table on that in one of my papers.

Q. Is this in the paper that was referred to this morning, which is number four in the binder and which deals with the Respiratory Cancer in Relation to Occupational Exposures...etc., etc., the one...

A. I think it's in the British Journal of Industrial Medicine paper that we talked about.

Q. Yes.

A. Look at...I think we just went over that paper.

Q. Yes.

A. Look at table four...

Q. Yes.

A. ...and look at the regression coefficients. The contribution of time at a fixed level, that regression coefficient is point seven three, and the contribution of level at fixed time, it's point seven seven. They are not very far apart, and I concluded that both time and level were making about equal contributions to the lung cancer excess.

I think, incidentally, MacDonald has a table like that, that comes out about the same place I did.

Q. I would like to...on the question of latency... I refer you to document number six in the binder, which is taken from your conclusion, and would like to ask you to comment on the question of the latency period, given the uncertainty of some of the background information that you used to compute in your cohort. I guess I only have the conclusion page. I would suspect that this is in connection with the work that you had done before, is that correct, sir?

DR. DUPRE: May I ask what article you are referring to, counsel? Is it, Pitfalls in Epidemiological Research?

MR. BAZIN: That's right, and I only have the last page. I don't know where it is.

UNIDENTIFIED SPEAKER: I have the whole thing here.

THE WITNESS: A. That's terrible. You've got to read the whole thing.

MR. BAZIN: Q. I have the conclusions, and I

Q. (cont'd.) would like to find out, have some comments from you on the question of the problems that exist for someone who is trying to build up statistics in examining what the latency period may be, given certain things that may happen or may not happen. I'm thinking, for instance, and you mentioned that the correction of death certificates and the failure to be precise in the exposure levels, and so on. Would you comment on that?

A. Well, latency now is usually defined as the time between first exposure and the appearance of a tumor. I don't know...and you could talk about that without talking about dose, if you want. So it wouldn't have...I don't think that would necessarily be related to dose if you just talk about latency.

I'm not sure...could you tell me....what is the question again?

Q. I'm trying to elicit from you comments on the question of your determination of what the real exposure may be, to determine eventually what may or may not be a proper latency period. You did mention that you may have to wait twenty, thirty, fifty years to find out some of the things about asbestos, so I'm just trying to elicit from you comments on this.

A. Well, once again, if you wanted to just talk about latency, the point you would have to determine is when exposure started. This is..if you want to dig into this concept you are going to find out it's not a very precise concept, because you don't know when the cancer starts either. You might count it between the time exposure starts, and the person dies, but the cancer could have started five years before death, or ten years. Nobody knows when these things begin.

If you think about this latency problem in relation to the dose rate, we just don't have very much data on that.

A. (cont'd.) That's just a theoretical construct based upon chemical carcinogenesis.

You might look, if you have the whole paper, figure one in that paper which kind of is a theoretical construct of what time to tumor would look like if the Druckery theory as modified by Jones and Grendon is actually true, and I think you see there that at those dose levels...I don't know if you all have this thing...it's got a series of lognormal curves. At the dose levels that I did the calculation for, the bulk of these cancers would be occurring in a normal lifetime.

Now, if I had been able to plot point one, which would have been hard to see on that thing, you would have thought perhaps that the time to tumor would have been away out at eighty or ninety, or something like that. There's a point at which we start to get beyond lifespan.

But let me just emphasize again, this is a theory. We'll have to wait to see if our experience fits this or not.

But, I think that is a graphic illustration of what I mean when I talk about the time-to-tumor /dose-rate relationship.

Q. The years to onset is years of exposure in this diagram?

A. No, no. This diagram deals with the effects of a single exposure for a full year, at different dose rates, and there is no way you can tell what the relative risk is from this and it is just simply a way of displaying the concept without extrapolating to actual...what actually is going to happen.

To go back to how this started, it started with the typical animal experiment where an animal is injected or intubated with some chemical, and then they wait to see what happens. Often times these are implantations, for example, so you get the dose - bang, you know. The dose just happens all

A. (cont'd.) of a sudden in these animal experiments like this, and they just simply observed that the greater the dose, the quicker the tumor appeared, and the lesser the dose, the longer it took for it to appear. These kinds of curves are the kind of curves you could generate from what we know in mostly chemical carcinogenesis in animals.

Q. Chemical?

A. Yeah.

I should say it also applies to...it seems to apply to radiation, there is a big debate about this. If you look at my references in the original paper, you find out I drew largely upon data from uranium miners, which drew largely on data from Hiroshima and Nagasaki, and that the shapes of these curves are mostly derived from this large body of epidemiologic data on radiation. So it has its birth in animal experiments with chemicals, and radiation mostly going back to the atomic bomb explosions in Japan in 1945.

MR. BAZIN: Those were the points that I wanted to raise, Mr. Chairman.

DR. DUPRE: Thank you, counsel.

Miss Jolley?

CROSS-EXAMINATION BY MISS JOLLEY

Q. I wanted to deal with your retiree study a little bit, because it has important implications.

You have indicated to us that the company that you studied was Johns-Manville, and obviously they were extremely co-operative. Did they also fund your study?

A. Oh, sure.

Q. Right. I just wanted to make that clear, because it wasn't clear in your article.

I wanted to deal with the whole issue this morning, and my level of sophistication in questioning may be a lot lower

Q. (cont'd.) than my colleague's to the right, but I want to deal with the issue of using a midget impinger, and you have indicated that there is a lot of variability. A number of the studies have indicated not much correlation between dust particles measured by midget impingers and the actual fibers contained in the samples. You used the conversion factor of three, which I gather is from Gibb's paper, is it? Or Gibbs did present that conversion factor.

A. There are a number of references on that.

Q. Right. Would you say that that is accurate enough that we should base the standard on now, that kind of conversion factor?

Can we use those epidemiological studies to indicate what membrane filter, phase contrast microscopy standards should be? Can we accurately do that?

A. I think so. I guess I would have to say kind of a qualified yes. Let's be realistic, what are you going to base it on? At one extreme you can say some data are better than no data. At the other extreme you can say, it certainly works. One of the striking things is that however bad the midget impinger was, it certainly predicted who was going to get lung cancer, and I think we talked about this a little bit this morning.

I think you must remember that while there is not a very good correlation between midget impinger counts and fiber counts. There is a correlation, it's point three or point four, and who says you are so great in counting fibers anyhow? There must be a fantastic variability in both variables. You certainly wouldn't expect a very good correlation if the fiber counting isn't very good. You've got a lot of noise in the fiber count, a lot of noise in the dust count, and I wouldn't expect a very good correlation in any event.

A. (cont'd.) So perhaps point three or point four isn't bad...considering that you've got two measures that...neither of which is very good. At least it's in the right direction, you know, it's positive. I mean, if it was negative, then you could have...that would be rather frustrating.

Q. I wanted to deal with some of your papers on actual fiber types. You make a number of assumptions about fiber type. Was there any measurement actually taken, or identification of the fibers in the air, or was it just purely associated with job description?

A. Well, we knew what the fiber content of the product was.

Q. Right.

A. I'm sure the assumption must have been that the mix in the air would be like the fiber content of the product. I don't know of any actual measurement of different types of fibers. In fact, I suspect that's been fairly recent that you could even make that identification.

Q. Were these job descriptions in actually different plants, and things like that?

A. Yes. Each plant, for each plant there were departments, and each department there were jobs, and for each job there was an estimate of dust exposure expressed as billion particles per cubic foot by year, going back to the beginning of the plant...in big matrix charts.

Q. Would your cohort that was exposed to amosite only, can you guarantee that they were exposed to amosite only?

A. That's what it said. I don't know. I don't think anybody is exposed to anything 'only', but I suppose relative...this is not a relative term...and the industrial hygienist, that was his estimate. Perhaps they probably would have been making insulation...I don't know if there are very

A. (cont'd.) often pure exposures. I think perhaps the Patterson, New Jersey plant that made insulation for the navy in World War II...it seemed like that might have been pure amosite, and there were operations like that, that Johns-Manville had. I would say it's probably not bad.

Q. The one thing I was concerned about was when you were discussing with Mr. Warren, you said that there was a cohort that was nonexposed?

A. Yeah.

Q. In our experience in the trade union movement, way back when, when these levels were taken, etc., and when this population was exposed, there's very few places in the Johns-Manville Plant, for instance in Toronto, that there was no exposure. Where would these workers be working?

A. Oh, these were not in asbestos plants. See, I did the entire Johns-Manville Corporation and they have plants that don't make asbestos. They make fiberglass and they do other things.

Q. I assumed it was just in the asbestos industry, sorry.

A. No, no.

Q. Okay. Based on your study though, would you recommend setting different standards for different fiber types?

A. I would sure give it some serious consideration. I would certainly look at the reasons for the British, for example, setting different standards, and I would look at the evidence very carefully.

Q. Why does the U.S. not take that track?

A. Well, when we say..I don't know. Why do I think they don't?

Q. Why do you think?

A. Oh, boy. That's probably...why do I think they don't...I don't know why. I don't understand them. I

A. (cont'd.) really don't. I don't know why they don't acknowledge...I don't know why Johns-Manville...let's start out...I mean, who doesn't believe there's a difference? Johns-Manville won't acknowledge there is a difference, number one.

Gee, I would think that would be to their advantage. I don't know why they don't.

The Selikoff group doesn't. I don't know...and the U.S. government.

Q. Is it not based on some of the animal data on carcinogenesis?

A. Perhaps, yeah.

Q. I won't pursue that then. I want to deal...

A. When you find out, I'll read your report.

Q. Okay.

I just want to know why you did not find any mesothelioma in your retiree study.

A. I think it's because there wasn't any to find.

Q. Had they all died beforehand?

A. Did you read the last paper? I have...I showed exactly why I didn't find any. I'll have to look at it to...let me think. First of all, there were quite a few cases in my retiree study, and I note that in my first paper. I say there were several cases, a number of cases under the age of sixty-five, so I did find some number..seven or eight or nine, I can't remember the number. And I do have a table that shows exactly how my cases compare with Burrows' cases, who did a paper, and...

MR. LASKIN: Page 125 of tab ten.

THE WITNESS: A. Yeah, let's see...I mean, I think that's the answer. I was obviously as curious as anybody else about that, and I made a special effort to find out, and...table seven, page 125, and I do have a conclusion. I found one case,

A. (cont'd.) and it was not at the Mansville plant, but then I missed a case that was at the Mansville plant, so back I had a case again.

Well, some of them were women, and women weren't eligible; some were...I think there was one or two people that were engineers, and we didn't study salaried people, just wage.

It is curious that these mesotheliomas seem to be occurring under the age of sixty-five, and I just don't understand that. Maybe somebody here does, but it's an odd thing. They do occur early and interestingly, they did cluster around the plant at Manville. I think...I don't know...of seven or eight, there were only two that weren't at Manville, and yet most of my cohort was not at Manville. Some of the big plants that Johns-Manville owns, or...not at Manville, in New Jersey, which suggests to me that if you find some people interested in mesotheliomas in a particular area, you'll find some. If you go out in places where there's nobody interested, the pathologists just don't report them. They don't get recorded.

So the mesothelioma thing is a little bit of how much interest there is and how hard you look, and do you know what to look for.

Q. I just found it interesting, because we had been told that mesothelioma had a longer latency period and so presumed that that was not borne out.

A. Yeah. It's a little bit of a mystery as to why they seem to be occurring younger. Somebody showed me a table here...you showed me a table, yeah...very strange...that you get the mesotheliomas at a much younger age than apparently the lung cancers, out of an asbestos plant.

Q. Moving onto the paper this morning, I guess I find it really difficult in that you say that asbestosis is just as fatal as lung cancer. That hasn't been our experience at Johns-Manville, for instance, here in West Hill. Most...well

Q. (cont'd.) a large percentage of the asbestosis victims, in fact, die of heart disease, according to our Workmen's Compensation study. Or is that Finkelstein, your study?

MR. FINKELSTEIN: I think most of them died of cancer.

MISS JOLLEY: Well, there was a quote in the Ministry...well, anyway, we'll get into that.

MISS JOLLEY: Q. But they do die of other things than asbestosis, and I just found that difficult to...why do you say that?

THE WITNESS: A. Well, I guess we have to decide what do we mean by asbestosis. I think disabling asbestosis...I mean where there's loss of compliance and breathlessness, is an extremely disabling and fatal disease. It's not a good way to go. I think this is kind of a hard...it's kind of a relative statement here. But historically, and mostly in perhaps other countries, there were asbestosis deaths observed. It was one of the fatal conditions arising from working with asbestos.

I don't know, I might modify that statement if you needled me on it.

Q. The other mathematical things that you did this morning, which unfortunately we didn't have the paper before us, so I'm just following along now, but you took the average census times three, to get the total population?

A. Mmm-hmm.

Q. Where did you come up with the times three?

A. Well, we've done perhaps seven or eight studies of industrial populations where we know the average census...I'll give you an example. I just did a smelter at Tacoma, Washington. The average census at the smelter has been about a thousand, for a good many years. I wanted to find out...I identified the number of different people who had worked a year or more at this smelter, starting in 1946, and

A. (cont'd.) the number who worked a year or more was three thousand.

So...I didn't know that when I started, I had to do a lot of work on that. Then I did some checking on other studies I had done where somebody said, we have an average census of five hundred and I would do this same thing and I would find that the number of different people who worked a year or more is fifteen hundred. Somebody says, I've got a plant with one hundred, and I found the different people was three hundred.

It suddenly occurred to me that the three seems to be the number that you multiply an average census by to get the number of different people who worked over a twenty, thirty year period.

It's obviously important to estimate this, and I thought it was better to do it empirically this way than...I don't know how other people estimate. It's a hard thing to estimate.

Q. It was just our experience that a lot of asbestos...there is a lot of turnover in asbestos industries.

A. Yeah, but I mean, this is mostly...and people work very short periods. I think that when you talk about the effects of asbestos, you really have in mind people who work perhaps a year or more, rather than people who may have worked a couple of weeks, and so forth...under most conditions.

Q. I wanted to deal with the smoking, because I was most interested in the...we have uranium mining in Ontario, and we were aware of what was going on in the uranium mining as to that, and I wondered if that would happen in fact in the asbestos as well, so that was interesting.

In your paper on smoking, or in your paper on that, you made a statement that there was a seventy-nine, seventy-seven percent, almost eighty percent probability that lung cancer was

Q. (cont'd.) due to asbestos, regardless of their smoking histories?

A. Mmm-hmm.

Q. That's correct?

A. Right.

Q. For purposes of compensation then, should people who smoke who get lung cancer, and have asbestos exposure, have their compensation reduced because they smoke?

A. No. I think my conclusion was that smoking is not relevant to the granting of compensation for someone who has been exposed to asbestos.

Q. That's not always the opinion in our Workmen's Compensation Board and other places..

A. I think there have been a lot of wrong decisions made on that, in both directions. I think there has been a lot of misinformation about that.

Q. As far as the policy of hiring nonsmokers, and things like that, that the industry is promoting, is there any relationship between smoking and mesothelioma?

A. I think there is, but I'm not really up on that. I think...I think there is. I can't answer that for sure. I think there is, but I'm not sure.

Is there? Somebody must know the answer to that. No one thinks there is?

MR. LASKIN: Selikoff, I think, suggests not.

MISS JOLLEY: That's what I had heard.

THE WITNESS: A. Well, I thought there was some British paper that suggested so.

MR. LASKIN: I suppose it all depends who you ask. There will be other people testifying.

THE WITNESS: A. Yes. Ask somebody else who knows more than I do about that.

MISS JOLLEY: Q. So when we are dealing with low

Q. (cont'd.) levels which may lead to mesothelioma, a nonsmoking policy will not protect those workers against mesothelioma?

A. Well, if it's true that smoking is not related, that's right, yeah.

Q. Similarly with gastrointestinal cancer? With a nonsmoking person?

A. Again, I don't know. I mean, I ought to know that, but I don't.

Is it? Somebody knows. Do you know? You'll have to ask somebody about that. It's a good question.

Q. Okay. I want to ask about intermittent exposures, because it's very important in terms of what is going on in our province right now. We've got a ripping-out of asbestos program going on, which I know went on in the U.S., etc., and we have a lot of brothers and sisters in the trade union movement who are being exposed to very high levels. In some cases they are protected, and in other cases they are not, and I think for an emergency problem we are very concerned that something be done to promote their protection immediately, because this is going on right at this present time. Is that a fair concern, a valid concern of ours right now? I mean these are intermittent, very, very high exposures, presumably?

A. I think it is. Until somebody comes up with some more data on it, I think I would be very concerned about high doses, even for short periods, and I think that's a tradeoff that you should think about very carefully when you decide to tear something out, particularly if it's not friable...in other words, if there is no flaking. I would think a long time about that.

Q. I mean, just as a comment, there are programs going on that are excellent, and that the workers are fully protected, and I think that's the kind of program we want to see.

Q. (cont'd.) I have just two last questions, and they are very short.

One question, and which I'm not sure that you want to address because you are not a medical doctor, but you make the statement in your epidemiological basis for the asbestos standard, when you are discussing the possibility of using x-ray changes. You say that x-ray changes have been mentioned, but these do not neccessarily measure the material impairment of health or functional capacity. This is probably manipulative of me, but our Workmen's Compensation Board uses x-rays to indicate impairment, and you would agree that that statement is correct, is it?

I mean, you would agree that your statement is correct?

A. I think in the context in which I made it, yes. I think that if you are looking for early changes, early signs of an effect, there appear to be some nonspecific x-ray changes showing up that we are not sure what they mean or what the consequences might be. In other words, I think if left along, probably none. I think that you might have an arguable case here if you tried to set standards based on some very, very early changes, and I'm thinking particularly of the kinds of changes that were observed in the community around Patterson, for example, and perhaps in some families of workers, as to what those...you know, it's not clear what those might mean.

In the context of the U.S. intent in law, whether you could use those to set a standard, somebody might make a good case against that.

Q. There's one last question I wanted to ask you, and that was in your 1973 study. Do you still believe that an SMR of a hundred and fifty, or a fifty percent excess lung cancer is not excessive?

A. Did I say that?

Q. Yes. I haven't heard any industry group ever say that.

A. I think that...to go back to something I said earlier...I don't think epidemiologic methods are good enough to pick those up with much reliability, and perhaps that's what I had in mind, that we are not...when you get down to levels like that, just little alterations in smoking patterns could affect that, and after all this is a really crude comparison group we are using. In other words, we are assuming, to take smoking, that people that retired from the asbestos industry have the same smoking habits as the U.S. population, and I just am uneasy about making much out of a one fifty SMR based on epidemiology.

MISS JOLLEY: Okay, thank you.

MR. STARKMAN: I'm just wondering if Dr. Enterline would like to take a five minute recess, or if other people would be interested in that?

DR. DUPRE: You will have...

MR. STARKMAN: I'll be brief.

DR. DUPRE: That may depend in part on the length of your questioning. The doctor wants to make the airport.

MR. STARKMAN: I would be very brief.

DR. DUPRE: Okay.

Would you like to take a brief break, Dr. Enterline?

THE WITNESS: I can stand it if you can.

MR. STARKMAN: All right.

DR. DUPRE: Proceed, doctor.

CROSS-EXAMINATION BY MR. STARKMAN

Q. Dr. Enterline, I would like to just deal initially with your paper, Proportion of Cancer Due to Exposure to Asbestos, where I believe after explaining it to us this morning you conclude that the increased risk is

Q. (cont'd.) approximately one percent, whereas the government report referred to estimates somewhere in the thirteen to eighteen percent range. I haven't seen the government report, but it's obvious this is an enormous difference. I was just wondering if you would comment on why you think this difference exists. In other words, in the methodology what was it that created this enormous difference in your results?

A. As I recall, the government assumed that there were several million heavily-exposed people alive today in the United States, and I think four million sticks in my mind. They took the findings from Selikoff's insulators, asbestos insulators, and said that these four million people will have the same mortality experience as the Selikoff insulators - seventeen thousand.

Now, remember Selikoff had essentially all the insulators there were. He had all the union members, and as I showed you in my census figures, he apparently had covered the field pretty well.

One question would be, who are the other people in this four million figure? They couldn't be insulators, because there were only twenty thousand, let's say. They can't be primary products workers, because there were only about twenty thousand of those - whatever I showed in that figure - less than a hundred thousand.

I can't imagine who the rest of these four million people were. They are not in any group that I can identify, or anybody else can identify. So I think one of the problems was, they greatly overestimated the number of heavily-exposed... heavily-exposed, I mean like insulators, like asbestos products workers, and of course if you take a large number like four million and you multiply the twenty-two percent excess, let's say, that Selikoff had, you get a very large number of cancer cases. I think that's one place they made a bad mistake in

A. (cont'd.) doing that, and that's one reason I spent a lot of time trying to decide how big is the heavily-exposed population, and I mean heavily exposed like the workers were upon whom we have data - insulators or products workers - and I can't find as many as a hundred thousand. So there's one reason for most of the difference, I think.

Q. It occurred to me that another reason might be that you chose as your...as the index disease, asbestosis?

A. Yeah.

Q. I was just wondering, before I go further, whether you can just define a little more for everyone here what it is you meant by asbestosis. You say someone dies from asbestosis, what does that really...what was the cause of death? I know it says asbestosis, but what really happened physically, physiologically, to the person?

A. Why does someone die of asbestosis?

Q. What does it mean when you say someone dies of asbestosis?

A. Well, the intended meaning in the field of public health is, that the condition that initiated the train of events that led to death was asbestosis, and these are the instructions physicians are given in filling out a death certificate.

From the public health standpoint, of course, the notion is that if you could prevent the condition that initiated the train of events that led to death, you would prevent the death, so that an old lady, let's say, who goes to hospital with a broken hip, and dies of pneumonia is coded as death due to broken hip.

Q. So this, in your figure that would then include people who died from mesothelioma?

A. It could, mmm-hmm.

Q. I'm asking you if it did?

A. Hmmm?

Q. In your study, would it include those people?

A. Well, first of all that's a rare disease and if a physician sees that, you know, eureka, it would, he would get that on, if he didn't get anything else on the certificate.

I have a study which I show what was actually on the certificates for asbestosis deaths. In one of my early studies I have a table that shows what the certificate actually showed. I think it's interacting with lung cancer though, in that particular study.

I have looked at these certificates, though, since we have done some of these followup studies. I have seen the certificates.

But there are a set of coding rules. This is pretty well worked out...at least we try to be consistent in assigning causes of death, and make sure people die of only one thing.

Q. What I'm really getting at is, if someone dies from a heart attack because their lungs are filled with asbestos fiber and the heart is overworked, would that go down as a heart attack or as an asbestos...

A. Asbestos. If a person died in, say, right-sided heart failure...or is it left-sided? Right-sided, due to pulmonary disease, say bronchitis or whatever, he would go down as a bronchitis death or emphysema death, or whatever.

Q. But not as an asbestosis death?

A. Well, say if he had asbestosis and he died of pulmonary hypertension...I don't know if it's...perhaps it is...he would go down as an asbestosis death. That's the rule, now what the doctors do...we can't always be sure they follow the rules, but we do everything we can to get them to follow the rules.

Q. I guess essentially what we are saying is,

Q. (cont'd.) there is no way to verify whether that is so, working off the death certificates?

A. Yeah, sure. We who work with these things have done a lot of studies on this, you know. We are reasonably sure the system works. We are reasonably sure that the rules are being followed, and great efforts are made to see that they are. We query death certificates, send them back to the doctor and say, you've put the wrong cause of death down.

There is a whole system to make sure the system is working.

Q. Well, on table three of that paper, where it says Other Asbestos Deaths, I know you multiply by one point five, but why would there be a necessity to do that if they were all included on the death certificates?

A. Table three? Other Asbestos Cancers?

Q. Yes.

A. Well, you see the estimate...when I got down to twenty-five oh one, I had accounted for all the lung cancers. Now I had a problem of what about the gastrointestinal cancers.

We don't have much data...I couldn't do that directly like the lung cancers, so I had to go back to my first method that I started out with and said well, what on average is the ratio of asbestos lung cancers to asbestos other cancers?

I found out that looking over a number of studies that for every two asbestos lung cancers you find, you are going to find one other asbestos cancer. By that I mean a cancer caused by the asbestos exposure.

So at that point I said, well, the best thing I can do is say well, knowing that from a number of studies, then the number of additional cancers in addition to the twenty-five 0 one caused by asbestos must be twelve fifty, half of the twenty-five 0 one...on a two-for-one basis based on looking at a lot of studies.

Q. The people that died from heart disease, are they on this table? Caused by asbestosis?

A. Sure.

Q. Where are they?

A. Well, wait a minute. This table is only about cancer. It has nothing to do with heart disease.

Q. Yes, that's what I think I'm trying to get at.

A. I'm sorry. I may have misunderstood you. The exercise was to see of all the cancers what proportion are caused by asbestos. If it had been of all the heart disease what are caused by...perhaps, you know, I could have done it differently.

Q. On the same table, I'm looking under the Nonoccupations Exposure, we have asbestos lung cancer - twenty-eight deaths?

A. Yeah.

Q. It's an insignificant amount, statistically, in light of the total population number. How would that number be arrived at?

A. Well, I looked at a couple of studies on ambient air levels of asbestos, calculated that across the entire U.S. population the average exposure is probably five nanograms per cubic meter, and just extrapolated down to five nanograms.

Q. If the average...if five nanograms across the population, the American population, results in twenty-eight deaths, then can I just bring us around to the lineal nature of the dose-response curve that we were discussing earlier, and particularly the area between zero and point five?

You say five nanograms causes twenty-eight deaths, and isn't there then...wouldn't there be a tendency to think that as you worked your way up to point five, the numbers would grow proportionately in there? Have there ever been any studies done on that?

A. To point five fibers per c.c.?

Q. Yes.

A. Oh, sure. I mean, let's see, the conversion factor is thirty thousand, I think? I can't remember. I think a fiber is worth thirty thousand nanograms.

This is really hairy stuff, you know. You've got to weigh the fibers.

I have a lot of data on hair dryers. The way I got into this was in the hair dryer business, and NIOSH actually weighed the fibers. It's the most fantastic thing they did, and I used their data to get from nanograms to fibers.

I don't know...five nanograms would be like ten zero...point ten zero something fibers, you know. It would be a tiny, tiny number of fibers.

Q. Yes.

A. But I had to convert nanograms to fibers, and then put fibers into my regression equation to come out with the excess relative risk.

Q. Do you have any idea if you make those conversions what the numbers would be like at point five?

A. Point five fibers?

Q. Yes.

A. Sure.

Q. Can you...

A. Well, I would have to work it out. What do you want to know? You want to know what would be the relative risk at point five fibers?

Q. Yes.

A. For how long? Fifty years, or...?

Q. On the same basis, yes. On the same basis to be consistent with what we have been discussing.

A. Well, you have to look at my paper on testing the standard, you'll have to pick out who you believe, and they'll

A. (cot'd.) work it out for you. If you want to believe me, or Peto, or...you know, what would the relative risk...?

Okay, let me think. What would the relative risk be at five fibers? I think it would be...point five fibers it will be one point 0 five. The relative risk will be a five percent excess at point five fibers after fifty years.

Is that right? Anybody disagree with that?

Boy, this is a good class, I'll tell you.

I'm going to start asking you questions pretty soon.

Q. Before we move off the subject, you mentioned that you were asked to review the government study. I am just wondering, who was it who asked you to review the government study? That there was thirteen to eighteen percent of all cancer which was caused by asbestosis.

A. Well, when I made that comment I said that I had been asked by the organizers of the Banbury Conference on.. what's the name of it...on the Quantification of Occupational Cancer. Okay, who are they? Gosh, I don't know.

Who ran the Banbury Conference? First it was... I think it was funded by NIOSH originally. I think it was initially a government-sponsored conference, isn't that right?

Let's see...who...I know Marv Schneiderman was on the planning committee, and I think Richard Peto. You know, I could resurrect how the thing started, and like most conferences, somebody got together and said well, who will we get to talk about this and get to talk about that. If you have ever run a meeting, you know how that happens.

Why did they pick me? I haven't the foggiest.

I do have an idea. I'll tell you why. When the government document came out, which may have been 1979 or so, the AIA asked me if I would be interested in commenting on it, and I was very interested not just from the standpoint of

A. (cont'd.) asbestos, but because it had a big section in there on chromium - chromate exposures - which I am very interested in, and I did write some comments and used a method to make an estimate that I presented here today, using the asbestosis cases as an index case. That comment of mine was then incorporated into a paper that was published by two people at NIHS, so those comments got kind of circulated, and I think that's perhaps the reason that I was asked to see if I could develop the method a little further for the Banbury Conference.

Q. I would like to look back on the drawingboard there where we were talking about smoking and asbestos, and I was wondering if you could comment on...we talk about the length of time that someone is exposed to asbestos dust or asbestos fibers, but what about the length of time they have been smoking? How would that enter into those calculations of the relative risks?

A. I don't think that's been worked out very well. I mean, it's a really good question that might...you know, there's an answer to that, but I don't know what it is.

I know that a very good question is, does it help to stop smoking, and I think the answer is, yes, it does. I think there is some data on that.

In other words, if you are an asbestos worker and you stop smoking, will it change the risk. I think the answer is yes, it definitely does.

Q. But does it change the risk any more substantially than it does in the rest of the population, should they stop smoking?

A. I don't know, but you are going to have some good witnesses. I'm sure they can tell you more about that than I can.

MR. STARKMAN: That's all. Those are all my questions.

DR. DUPRE: This completes the cross-examination, I gather, counsel?

MR. LASKIN: At last, time for the Commission.

DR. DUPRE: All right. Still a round for the Commission.

Dr. Uffen?

DR. UFFEN: Mr. Chairman, I have a couple that have arisen during the day. I think they are quite short.

Could I come back to one that was raised just a little while ago about the death certificates? Quite recently we were told that as high as thirty percent of the death certificates may have to have been altered after an autopsy. To a layman, that seems like an enormous figure, thirty percent. Does that seem reasonable to you that it's true, or close to the truth?

DR. ENTERLINE: It seems a little high to me. But autopsy for what? I mean, autopsies are often done where the cause of death is in doubt, and...

DR. UFFEN: The circumstances here were, a union questioned the circumstances and requested an autopsy, and subsequently the death certificate was altered. I put the question, to what extent does that occur, and the answer came back, as much as thirty percent of the time.

DR. ENTERLINE: I think in that situation that probably would be about right. Where there was...see, to begin with, you are starting out with a question. In other words, there must have been something peculiar about the case to raise the issue, and so I would guess in those situations you might get...

DR. UFFEN: They were probably Workmen's Compensation cases.

DR. ENTERLINE: Yes.

DR. UFFEN: I would like to go to the two papers

DR. UFFEN: (cont'd.) that you brought in today. The first one, I found it admirable to see your logic in trying to estimate all the parameters that were needed to make a projection into the future and to double-check them and so on.

I waited right to the end to see whether, after you had done your best estimates, if you would also do worst or extremes. Have you done extremes? Have you gone through the exercise and said, let's put in the worst possible, conceivable case and what does that do to that one percent figure at the end? If it changed it to five percent, I would think that was quite significant. If it changed it to one and a half or to two, it would be quite different.

Have you done that?

DR. ENTERLINE: No. I try to avoid that because people somehow have the idea that making worst estimates in this situation is going to help somebody. I think in this situation you are talking about an allocation of national resources among people that are competing with each other, and you want to make the best allocation you can among competing things. I think to overestimate one, or overestimate them all...what if everybody overestimated? Of course then you find out that you could account for three hundred percent of all the cancers in those... the guy that makes the...the chromium people say that twenty percent of the cancer is due to chromium, and the nickel people say twenty percent is due to nickel, and the cigarette people say ninety percent is due to cigarettes. I think then people that are rule making are really confused by that.

Everybody comes in and says, look at me, my problem is worse than his problem. I think they ought to stop doing that. You know, I think...let's do our best, let's get a best estimate and try not to make a mistake on either side and see what is our best estimate. I tried to do that.

I think for this particular purpose, that's

DR. ENTERLINE: (cont'd.) the thing to do. If I were...

DR. UFFEN: I was thinking from a point of view of sensitivity analysis. It's usually desirable when data are not precise to have some estimate of the sensitivity to...

DR. ENTERLINE: The range.

DR. UFFEN: That was the concept...

DR. ENTERLINE: Yeah, yeah. Well, I'm afraid how that's used, because one you do that then people...one group uses the upper estimate and the other group uses the lower thing, and then you find out that they are fighting with each other.

DR. UFFEN: Unfortunately, we are going to hear more.

DR. ENTERLINE: You are going to hear them all.

DR. UFFEN: We are faced with the chore of deciding.

Can I move to another one? It's your second paper where you were, I think, examining the initial for it, the excess of lung cancer.

I'm right...if I understood it correctly...you did the whole works for two fibers per c.c.?

DR. ENTERLINE: Yes.

DR. UFFEN: Have you done it for one fiber per c.c., or one and a half or anything else?

DR. ENTERLINE: Sure. I mean, all you do, since this is a totally linear system, if you want it for one, just divide my estimates by half. If you want it for point five, just divide it by four. Somebody had asked the question...see, I calculated that at two fibers for fifty years there would be a twenty percent excess...and he asked, what about point five fibers for fifty years...that's a quarter of two...and I said a five percent.

So since the system is totally linear, you can divide by any number.

DR. UFFEN: You are all right as long as we recognize that this is assuming a linear dose-response curve?

DR. ENTERLINE: That's right, yes.

DR. UFFEN: But as we get to lower fiber counts, one point five, point one or whatever it is, is my understanding correct that the reliability of that linearity assumption is questionable?

DR. ENTERLINE: I think so, yes.

DR. UFFEN: So we mustn't just divide by two if we go from two percent to one percent...or two fibers per c.c. to one fiber per c.c.?

DR. ENTERLINE: Well, I don't know any other...I don't know anything else to do, is the problem. I don't know how else to do this, but to divide like that.

If you...you know, I could perhaps defend doing that division. I don't want to overemphasize this little curve. That's a theory.

It depends what you are going to do with the data. I don't know what you are going to use this for.

DR. UFFEN: As a layman, what is putting...what is being put to me is just that...should we have a regulation that has two fibers per c.c, or one, or point one. And as I listen to the experts, I find the basis for making a rational judgement clouded because there isn't any, either agreement by experts, or data which can be trusted. That's all I want to find out, to what degree can we make these assumptions and know that we are on firm ground?

DR. ENTERLINE: I think you should simply at this point make linear extrapolations.

DR. UFFEN: Linear?

DR. ENTERLINE: Yes.

DR. DUPRE: Dr. Mustard?

DR. MUSTARD: In your exhibit six, and in some of

DR. MUSTARD: (cont'd.) the other ones, you get back to this cause of death question, and I would like to ask the question a slightly different way than others have been asking it.

As an epidemiologist you are dependent upon what the system out there identifies, and under the classification of respiratory disorders as a cause of death, it isn't a huge figure, it's a reasonably respectable figure of four or five percent in this country.

Has anybody taken that classification, the broad classification of respiratory disorders, and gone at it to find out in a kind of random, in a properly controlled way, as to how much asbestosis is not diagnosed within that classification, and I suppose the way you would have to do it is, someone have to construct a study in a center where you could get post mortems in in a random fashion to find out if there is a certain amount of chronic chest disease that is diagnosed as a matter other than asbestosis, which should be diagnosed as asbestosis.

Do you know, do you have any feeling if there is an underestimate, and if so, what it is?

DR. ENTERLINE: I guess there is an underestimate. In one of my papers we add in the category, pulmonary fibrosis, and assume that those cases should have been called asbestosis because they occurred in asbestos workers.

But I think...you get down to this question of what is asbestosis. Do you mean any fibrotic change in the lung of someone exposed? Do you mean the presence of fibers? Do they have to have fibers? I think people have struggled with this somewhat and it's a kind of a tough question because the parameters aren't terribly well defined.

The closest I could come, I think, was to add in this pulmonary fibrosis subcategory that was listed on death certificates, and it adds about a third to the deaths, if you add that.

DR. MUSTARD: But you are not aware of anyone having tried to determine scientifically if there is an underestimate, and the size of the underestimate?

DR. ENTERLINE: Of asbestosis? Now, you have to define whether you mean the presence of the disease at time of death...you know, there is a difference between dying with and of, you see. We are mostly interested in dying of.

There is another kind of question of sort of the prevalence of disease in corpses. I mean if someone dies, there may be a lot of things wrong with them, but I think you have to go back to the old notion in public health of cause/effect, preventing death, and there we are mostly interested in died of rather than with, although we recognize that the concurrence of conditions precipitate deaths and there is no way to know what the contribution of each of them is. It's a tough question.

DR. MUSTARD: I think dying of chronic chest disease that is fibrotic in nature, in which asbestosis may or may not have been present. That's what I'm trying to get at. That, I think, is an area in which we have some uncertainty.

DR. ENTERLINE: I think, yeah.

DR. MUSTARD: Okay, my second question is related to the...some of these are exhibits, but it's on page 177 of exhibit seven...in which you have your projected curves in relation to latency in relation to dose. How confident are you that those curves bear some relation to reality?

DR. ENTERLINE: That's the curves there, you are talking about?

DR. MUSTARD: Yes.

DR. ENTERLINE: Well, I think they bear some relation to reality. If I was really confident of those, I would do more with that concept. I think it's an idea whose time hasn't quite come yet, and it's like all these things, ideas wax and wane, and this one is sort of just simmering and

DR. ENTERLINE: (cont'd.) waiting for something to be done with it.

DR. MUSTARD: Well, let me pose a practical question to you. Let us suppose that I have been working in an asbestos plant and exposed to asbestos, and cancer becomes diagnosed at, say, nine point two years of exposure, but my compensation arrangements in my particular jurisdiction are that you have to have ten years of exposure to be eligible. Those curves would give me some problem, given the ten year cutoff, because you suggest there is a slow slope and then it accelerates. I guess the question I would like to pose to you in view of the uncertainty there is, in your view do you think that having a specific cutoff is sound scientifically at this stage, or should one be more flexible in one's interpretation of the cutoff time in terms of the period of exposure and developing cancer?

DR. ENTERLINE: Oh, I think that's only one of the two factors. The other is the intensity of the exposure.

DR. MUSTARD: Yes, good exposure.

DR. ENTERLINE: It's high, very intense exposure?

DR. MUSTARD: Oh, let's take your middle curve there.

DR. ENTERLINE: Well, now, that's time to tumor. What's the median...

DR. MUSTARD: This is, you get your fourth level of exposure.

DR. ENTERLINE: Yes. Well, you could...that curve in this particular case, you could make a probability statement as to whether or not that particular disease was due to that particular exposure by reading off that curve. If... let's say...no, I think that there is a difference here.

You are talking about...that curve is about when the disease appears in relation to when the exposure started, and it doesn't say anything about the duration of exposure, that

DR. ENTERLINE: (cont'd.) particular curve.

That particular graph deals with the situation where people get...in this case a year's worth of exposure at different levels, and displays when the cancer is going to appear in relation to when that year's exposure took place. That's what that graph is.

DR. MUSTARD: But if I start back at nine and a half years ago, and I'm getting exposed continuously, although all the way along, surely that curve is still applicable because I've had my one year, plus? I'm just concerned about trying... your view as a statistician of the decision-making rule which you have to apply in taking arbitrary cutoff times, in view of the fact that the curve isn't a sharp change.

DR. ENTERLINE: Well, you said it. I think it's purely an arbitrary cutoff time. I mean, they couldn't be anything but arbitrary, and there is no reason it couldn't be nine or eight or ten or eleven. I mean, you're going to have to develop some kind of reason for having ten. I don't know what the reason is, but I would be worried about that.

DR. DUPRE: Dr. Enterline, what kind of study led to the indication that there might be a downstream effect among nonsmoking uranium workers? Was this a study of retired workers, for example, similar to the one you've done with asbestos workers?

DR. ENTERLINE: No, it was a study like the Dement study and others referred to, where the dose and the time were all mixed up, as I described earlier. You know, I think in that sense there is some controversy about those kinds of studies. It was not of retired workers.

DR. DUPRE: Are you continuing to follow your cohort of retired J-M workers?

DR. ENTERLINE: I'm not, no.

They could be followed anytime. That is, you

DR. ENTERLINE: (cont'd.) could pick it up any year you want, and clear this with our Social Security system and pick up the deaths. I think probably in four or five years when we think enough more deaths will have occurred, we will probably just run them through again.

This is not sponsored research at all, but in the university we have little pots of money that you can do this, put students on them. Students have traineeships and scholarships, and it's kind of a learning project for students to do that. We'll probably do this in a few years.

DR. DUPRE: Following up that cohort of retirees, would that be something that would indicate whether the uranium mining nonsmoker downstream effect is going to materialize among asbestos nonsmokers?

DR. ENTERLINE: You would have to get smoking histories on your retirees. See, we never had that.

DR. DUPRE: That you didn't have.

DR. ENTERLINE: Yes. Now, Selikoff someday will know this. I mean, if the history in uranium mining applies to the asbestos work, as we move on in the Selikoff study, and that's what that's based upon, he ought to see if this is true or not.

I have...we have one of our professors named Ted Radford is very interested in this phenomena, and has been pressing people in the asbestos field to see if it might not be true in asbestos as well as in uranium miners, just the downstream effect that we have to wait and see in nonsmokers.

He thinks that in the long run in uranium miners, smoking has turned out not to be an important variable.

DR. DUPRE: Counsel?

MR. LASKIN: I don't have any more questions for this witness. I don't know if anybody else at the table has any more questions. If not, I would certainly like to thank

MR. LASKIN: (cont'd.) Doctor Enterline...

DR. DUPRE: Well, I think Mr. Stewart would like to ask a question, from the Energy and Chemical Workers Union.

MR. STEWART: I have, Dr. Enterline, two points that I would like to raise concerning the study of the retirees. I attended a national pension conference in Ottawa recently, and the statistics that we were provided reflected that less than half of the people, the workers in this country covered by private pension plans, actually received pensions. So that applied to Johns-Manville, and that would simply mean that less than half of the Johns-Manville employees would reach retirement age and receive a pension.

Given the high morbidity and mortality rate at Johns-Manville, it would be reduced significantly above that. Now, that's one of the problems.

The other problem is that we do worker beside worker, one who may have a susceptibility to asbestos come up with asbestosis or lung cancer, and nothing happen to the other guy.

Now, the person that makes it through to sixty-five obviously has a predisposition against disease, rather than one toward disease, so really is a study of retirees significant in trying to determine the magnitude of the problem of disease in asbestos workers? Given those two points that I raised.

DR. ENTERLINE: I think that's a fair question. I think that...we talked a little bit about the mesotheliomas, for example, and I think we, for some reason, are missing those. I don't know why.

But we certainly weren't missing the cancers, though. I mean the cancers are there and there is a good, sharp dose-response relationship. You have to realize that while it's clear what the selection factor is on retirees, that also applies to any other study that is done. If I were

DR. ENTERLINE: (cont'd.) to have studied workers, say twenty years or more after first exposure, in other words I leave out all the people who die at less than twenty years, I would have also had a selection factor.

That is, anytime you select a restriction on the data...such as I only want to see people after twenty years from first exposure...you have introduced the kind of bias you are talking about because some people will have died at nineteen years and they will be out of your study, and you will say, gee, I don't know what kind of bias that had, but I think I've made a kind of a compromise and I think that my restriction makes a better study and I'll have to sacrifice losing the nineteen year death.

So I think anything in epidemiology does have to make some kind of compromises, and I feel that the fact that these people terminated their exposures, and they don't have exposure continuing, perhaps is on the plus side.

I realize there's some negatives. I don't know how to do a perfect study without experimentation, which you can't do. I would like...I think if I had my way I would like to study all the asbestos workers. It just didn't happen to be the kind of data I could get my hands on at the time.

MR. STEWART: The other point that I wish to make, and this concerns the statement about intermittent exposure, in 1970 Dr. Selikoff shared the research that he...with us, that he did with the insulating union. He did make a prediction at that time that we would see the same problems at Johns-Manville here in Toronto, that the insulators experienced.

I can tell you that it was worse. That the experience that we had in Toronto is a worse experience than showed up in his research conducted among the insulators.

DR. ENTERLINE: I saw those data.

MR. STEWART: He predicted it would be that bad, and actually it's worse.

DR. ENTERLINE: I think that's absolutely remarkable data, and I hope that gets in the world literature.

We, of course, can only go on what people are willing to publish or that we know about. I've seen those data. They are remarkable data. Absolutely remarkable data. I don't know of anything quite like that in the literature.

I think it would be worth looking more at that and see what was it...he got a ninefold relative risk, or something, you know. That's...very few people can ever observe that. It means...it has to mean that..almost half your deaths must be lung cancer deaths.

DR. FINKELSTEIN: Lung cancer plus mesothelioma.

DR. ENTERLINE: Yes.

MR. LASKIN: Can you help the rest of us and tell us a little more specifically what data we are talking about, and what period of time?

DR. ENTERLINE: Dr. Finkelstein showed this to me at the break. I had not seen this before.

MR. WARREN: Don't you think it would be useful if we had this as an exhibit so that everybody sitting here would be in a position to deal with it, rather than talking about it on the transcript?

MR. STEWART: I'm not relying on Dr. Finkelstein's data for my observations. These are actual observations that we have made ourselves, and it doesn't come from the data, although the data supports the statement that I made.

DR. DUPRE: Dr. Finkelstein, can you tell the Commission about the eventual availability of this unseen data that has just been the subject of these very interesting remarks?

DR. FINKELSTEIN: No, I can't. I'm not in a position to make a decision.

DR. DUPRE: You are not talking about data that is in a study in progress? There is apparently a study that has been completed?

DR. FINKELSTEIN: No. There is a study in progress. Half of it has been completed. I have finished looking at men who have been employed nine or more years. I am currently tracing the men who have been employed one to eight years.

So presumably relative risk, I include men with (inaudible)...but hopefully within the next six months...

DR. DUPRE: This is a study by the Ministry of Labour in this jurisdiction?

(REPORTER'S NOTE: No audible response.)

DR. DUPRE: Fine, that's what I wanted to ascertain.

Mr. Laskin, any more questions or comments?

MR. LASKIN: None, Mr. Commissioner.

DR. DUPRE: Dr. Enterline, sir, may I thank you very, very much indeed not only on behalf of the Commission, but I'm sure on behalf of the parties as well, and hopefully, eventually, on behalf of the Ontario public. You have been most generous with us, sir, and I thank you.

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THE INQUIRY ADJOURNED

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Edwina Macht
EDWINA MACHT